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SCALABLE TARGETING OF SOCIAL PROTECTION: WHEN DO ALGORITHMS OUT-PERFORM SURVEYS AND COMMUNITY KNOWLEDGE?

Emily Aiken
Anik Ashraf
Joshua Blumenstock
Raymond Guiteras
Ahmed Mushfiq Mobarak

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Scalable Targeting of Social Protection: When Do Algorithms Out-Perform Surveys and Community Knowledge?

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ABSTRACT

Innovations in big data and algorithms are enabling new approaches to target interventions at scale. We compare the accuracy of three different systems for identifying the poor to receive benefit transfers— proxy means-testing, nominations from community members, and an algorithmic approach using machine learning to predict poverty using mobile phone usage behavior — and study how their cost-effectiveness varies with the scale and scope of the program. We collect mobile phone records from all major telecom operators in Bangladesh and conduct community-based wealth rankings and detailed consumption surveys of 5,000 households, to select the 22,000 poorest households for $300 transfers from 106,000 listed households. While proxy-means testing is most accurate, algorithmic targeting becomes more cost-effective for national-scale programs where large numbers of households have to be screened. We explore the external validity of these insights using survey data and mobile phone records data from Togo, and cross-country information on benefit transfer programs from the World Bank.

Emily Aiken
Carnegie Mellon University
eaiken@andrew.cmu.edu

Anik Ashraf
Ludwig-Maximilians-Universität Munich
Department of Economics
anik.ashraf@econ.lmu.de

Joshua Blumenstock
University of California, Berkeley
jblumenstock@berkeley.edu

Raymond Guiteras
North Carolina State University
Department of Economics
rpguiter@ncsu.edu

Ahmed Mushfiq Mobarak
Yale University
Department of Economics
and NBER
ahmed.mobarak@yale.edu

1 Introduction

Hundreds of billions of dollars are spent on social protection programs and humanitarian aid each year (ILO, 2021), so accurate targeting of these benefits is vital (Hanna and Olken, 2018). Most programs rely on in-person data collection methods — such as survey-based eligibility verification and community selection of beneficiaries — to allocate transfers. These in-person targeting approaches can be expensive to implement, and in many cases still result in targeting errors that cause large portions of eligible beneficiaries to be excluded erroneously: Coady et al. (2004) find that a quarter of poverty-targeted programs in low-income countries are regressive (providing more benefits to rich households than poor).

Novel data sources and advances in artificial intelligence have created new opportunities for deploying algorithms to identify beneficiaries remotely, lowering the implementation costs of targeting (Aiken et al., 2022; Mukerjee et al., 2023; Lopez, 2020; Smythe and Blumenstock, 2022; GiveDirectly, 2022). These new approaches – using, for example, metadata on users' mobile phone usage patterns or satellite images of their homes and neighborhoods — are attractive because digital data can be obtained at a fraction of the cost of traditional in-person visits, and are more easily scalable if very large numbers of people need to be screened. Furthermore, these may be the only available data sources for remote and insecure regions where in-person surveys are prohibitively expensive or infeasible.

This paper compares a variety of approaches for identifying beneficiaries for a cash transfer program in southern Bangladesh, to systematically explore if and when it is preferable to target using digital data and machine learning instead of more traditional methods like proxy means tests (PMT) and community-based targeting (CBT). These comparison exercises required us to conduct a census of all 106,000 households in 201 villages, a household survey of a representative random sample of 5,000 households from 180 neighborhoods that included collecting detailed consumption data, and community-based targeting exercises in each of these neighborhoods. To deploy algorithmic

targeting, we also obtained the complete mobile phone call data records from all consenting survey households. We partnered with GiveDirectly to deploy substantial transfers of 30,000 Taka (roughly 300 USD, or 955 USD PPP) to 22,000 households using the phone-based targeting method, and additional transfers of 1,100 Taka based on the CBT exercise. We also collect endline data on household satisfaction with the targeting process.

We use these datasets to compare PMT-based, community nomination-based, and phone-based poverty targeting approaches. The PMT involved predicting consumption poverty from characteristics collected in our household survey. Phone-based targeting used phone usage behavior extracted from users' call detail records to predict consumption poverty. CBT involved asking community members to nominate the poorest households in their neighborhood through group meetings. We develop and analyze measures of 'targeting accuracy' achieved by these methods. Accuracy is measured by benchmarking against 'consumption poverty' - as identified through intensive consumption expenditures surveys. We also compare the relative costs of deploying the three different strategies based on detailed cost information recorded during data collection exercises. The comparison reveals a key trade-off between the accuracy and cost of traditional versus algorithmic targeting: while the PMT is more costly than phone-based targeting, it is also more accurate. And both methods out-perform the CBT in terms of *both* cost and accuracy.

We introduce a framework based on the simple idea that lowering the cost of beneficiary identification leaves more funds for transfers, to provide policymakers and administrators of social protection programs guidance on the conditions under which algorithmic versus traditional approaches to targeting should be prioritized. Relative costs of alternative targeting approaches vary with the scale of the program, so the analysis has implications for scalability. We supplement these new data from Bangladesh with existing survey data and mobile phone records from Togo to explore the external validity of our insights on the key tradeoff we identify between cost and accuracy.

Our first main finding — focusing on *accuracy* (not cost) — is that phone data based algorithmic targeting more accurately identifies consumption-poor

households than the community targeting approach we test. However, both methods are substantially less accurate than proxy-means testing. Other survey-based targeting approaches — including the Poverty Probability Index (PPI, Kshirsagar et al., 2017) and a decentralized peer rankings approach where people are asked to privately rank peers' poverty status — also outperform community-based targeting and are comparable to phone-based targeting.

Our second main finding — focusing on identifying the optimal balance between targeting accuracy and cost — is that the welfare-maximizing targeting approach for a specific social protection program depends on its scale and scope. We adapt the social welfare framework introduced by Hanna and Olken (2018) to account for targeting costs, and use this to compare the simulated welfare effects of community-based, phone-based, and PMT-based targeting approaches in both Bangladesh and Togo. We show that for programs with a relatively small budget that screen a relatively large number of households for eligibility, phone-based targeting is the most cost-effective. For programs with larger budgets relative to the number of households screened, proxy-means testing is more efficient. Community-based targeting, which is both more expensive and less accurate than phone-based targeting, is never the most efficient targeting approach in these settings.

This paper is related to three main literatures. First, there is extensive past work measuring the accuracy of various "traditional" approaches to targeting social protection. This literature finds that proxy-means tests are generally more accurate at identifying the consumption-poor than CBTs (Alatas et al., 2012; Basurto et al., 2020; Premand and Schnitzer, 2021; Schnitzer and Stoeffler, 2022; Trachtman et al., 2022; Sumarto et al., 2025). We also find that the PMT outperforms CBT in Bangladesh. Yet other papers have evaluated alternative approaches to identifying poor households, including geographic targeting (Baker and Grosh, 1994), "scorecard" approaches like the Poverty Probability Index (Kshirsagar et al., 2017), decentralized community-based targeting based on peer rankings (Alatas et al., 2016; Beaman et al., 2021; Trachtman et al., 2022), and random targeting via lotteries (Bance and Schnitzer, 2021). We provide head-to-head accuracy comparisons for *all* these approaches,

but importantly and distinctively, we add phone-based algorithmic targeting to the comparison set. This is an important addition, because the rapid spread of mobile phones in otherwise-data-poor regions of developing countries, coupled with advances in computing and algorithmic techniques, makes cell phone records a promising instrument for cost-effectively improve targeting of humanitarian aid in large scale.

Second, our paper adds to a small but growing literature that explores how "big" digital data sources can be used for targeting (Aiken et al., 2022, 2023c; Smythe and Blumenstock, 2022). Two prior studies in Togo (Aiken et al., 2022) and Afghanistan (Aiken et al., 2023c) develop the basic methodology underlying the phone-based targeting approach we deployed in Bangladesh. This paper goes further by setting up a direct comparison in the field against the increasingly popular 'community-based targeting' (Sumarto et al., 2025), which had not been previously done. In contrast to the prior literature, we go beyond 'accuracy comparisons' to identify the circumstances under which phone-based targeting is most efficient.[1] This allows to systematically explore the scalability of algorthmic approaches to targeting.

Finally, this paper contributes to a nascent literature on cost-effective administration of social protection and humanitarian aid programs in low-income countries. While development programs are frequently evaluated using a cost-effectiveness metric (e.g., Murray et al., 2000), there isn't much systematic evidence on the relative cost-effectiveness of alternative targeting approaches, which is what we attempt to provide here. The tradeoff between cost and accuracy of program targeting we highlight determines cost-effectiveness (Dutrey, 2007; Devereux et al., 2017). Two other studies measure cost-effectiveness of targeting relative to universal distribution: Houssou and Zeller (2011) and Hanna and Olken (2018). A novel contribution of this paper is to provide head-to-head comparisons of the cost-effectiveness of multiple popular approaches to poverty targeting at various program scales.

[1]Also related are papers that show how poverty can be estimated using non-traditional data such as satellite imagery (Jean et al., 2016; Yeh et al., 2020), internet data (Fatehkia et al., 2020), mobile phone records (Blumenstock et al., 2015; Blumenstock, 2018), and administrative records from financial services companies (Engelmann et al., 2018).

2 Data and Methods

The primary empirical context for our analysis is a cash transfer program we developed in partnership with GiveDirectly and the Government of Bangladesh in 2023. The program provided cash transfers of 30,000 BDT (955 USD PPP) to 22,000 households in three sub-districts in southern Bangladesh — Ramu, Teknaf, and Ukhia.[2] The cash transfer program was designed to target the poorest 21% of households within the program area. Our main analysis compares proxy-means testing (PMT), community-based targeting (CBT), and phone-based targeting (PBT) for identifying the consumption-poorest households in this setting. A timeline of the project is provided in Figure S1. In supplementary analyses, we use data from a cash transfer program run by GiveDirectly and the government of Togo in 2021.

Our analysis of targeting in southern Bangladesh relies on four main sources of data:

- A **census** of all households in 201 randomly chosen villages from the three study sub-districts in Bangladesh. This accounts for roughly two thirds of the households and villages in these sub-districts.[3] The census was conducted in February and March 2023. We collected phone numbers of all adult household members, and basic information about household characteristics and asset ownership necessary to compute the Poverty Probability Index (PPI).[4] The census collected information for approximately 106,000 households. This census was also used by GiveDirectly to register potential beneficiaries for their cash transfer program.

- A March 2023 **household survey**, which collected consumption expenditures, demographics, assets, and peer rankings. In this survey, we adopted

[2]The program was targeted to communities that host Rohingya refugees. These sub-districts host large refugee populations, and there is a sentiment that these poor communities deserve some support for hosting refugees in their midst.

[3]Based on the official 2011 census, we estimate that our census covered 65% of households and 63% of villages.

[4]The PPI for Bangladesh is available at https://www.povertyindex.org/country/bangladesh. See Kshirsagar et al. (2017) for PPI methodology and assessment in Zambia.

the standardized consumption module from the 2016 Household Income and Expenditures Survey (HIES) implemented by the Bangladesh Bureau of Statistics. Following the instructions published by the Bangladesh Bureau of Statistics (Ahmed et al., 2019), we use these data to construct a measure of per capita **household consumption expenditures**. The household survey was conducted with a representative random sample of 5,006 households from 180 neighborhoods in the study area. Neighborhoods were selected randomly from among the 890 neighborhoods enumerated in the census, stratified by upazila, neighborhood size (based on neighborhood size terciles), and the share of households in the neighborhood that were a religious or ethnic minority (no minority households vs. less than 10% minority households vs. 10% minority households or greater). Descriptive measures and summary statistics from the household survey are provided in Figures S2 and S3 and Table S1. We also included a **peer rankings** module in the household survey, based loosely on the mechanism of Bloch and Olckers (2022). For this module, we asked each household about eight randomly selected households in their neighborhood. They were asked to report how well they knew the household and to rank each household both in absolute terms, as well as relative to the seven other households on their list (details in Appendix A.7).

- Household wealth rankings from **community-based targeting exercises** conducted in November 2023 in each of the 180 neighborhoods. Our CBT exercises assembled 12-25 community members from all walks of life from each "neighborhood" to collectively identify the 20% households with the lowest socioeconomic status, who would later receive a one-time cash transfer of 1,100 Taka ($35 USD PPP). We adopted a protocol regularly implemented by BRAC to determine beneficiaries for their own social safety net programs, which is described in detail in Appendix A.2.

- Complete **mobile phone metadata** from all consenting survey respondents from March to July 2023, including records of calls, texts, and mobile data usage. These data were obtained from all four mobile network opera-

tors active in the survey region. Following the data protection procedures described in our IRB protocol, we pseudonymized or removed all personally identifying information, including phone numbers, prior to analyzing mobile phone metadata. Details on these protocols, and special considerations regarding data privacy and ethics, are discussed in Appendix A.8.

We use these data to assess several approaches to targeting social protections in the context of southern Bangladesh. We study three main targeting methods:

1. A **phone-based targeting** approach that uses machine learning methods to predict consumption expenditures from 1,578 statistics on each subscribers' mobile phone use (including information about calls, texts, contact diversity, mobility, and mobile data usage). Our machine learning methods are similar to those used in past work (Aiken et al., 2022, 2023c,a) and detailed in Appendix A.1. In short, we first obtain pseudonymized mobile phone records from all four mobile network operators active in Cox's Bazar, for all phone numbers from all consenting surveyed households. These data included metadata (including pseudonymized identifiers for the caller and recipient, date, time, and duration of calls, and GPS coordinates for cell towers used) for all incoming and outgoing calls and SMS messages placed between March 1 and July 31, 2023, as well as information on daily mobile data usage. From these data, we calculated 1,578 "features" describing mobile phone use for each pseudonymized phone number in the dataset[5], including statistics on call and text frequency, heterogeneity in contact networks, recharge patterns, mobility traces based on cell tower usage, and more. Finally, we matched mobile phone features to the household survey (for the 94% of households that provided at least one phone number that was present in the mobile phone records), and used the matched dataset to train a gradient boosting model[6] to predict log per-capita consumption

[5]Subscriber-level statistics on mobile phone use are calculated using the open source python library cider.

[6]A gradient boosting model is a nonparametric ensemble machine learning approach. The ensemble consists of a number of decision trees, each of which is trained to predict household poverty from the phone data features, and includes explicit regularization. The

using mobile phone features. Table S2 shows the phone features that turn out to be the most predictive of consumption in our Bangladesh data.

2. The **community-based targeting (CBT)** rankings from each community are used directly to identify the most deserving recipients in their neighborhood. See Appendix A.2 for details. Rankings are normalized within each community to a 0-1 range for consistency across communities. This approach implicitly assumes that wealth ranges are consistent across neighborhoods; a more sophisticated approach could make use of data on neighborhood-level poverty to adjust rankings.

3. The **proxy-means test (PMT)** estimates poverty status using verifiable assets and household characteristics. In our household survey, we collected information on 45 covariates that are common to many PMTs (Hanna and Olken, 2018; Brown et al., 2018), including household characteristics (for example, the number of rooms and the material of the roof), demographic information (e.g., the household size and gender of the household head), and asset ownership. We then used modern machine learning methods to develop a PMT that predicts log per-capita consumption from those 45 covariates (see Appendix A.3 for details). We expect that this represents a "best case" PMT, since many real-world PMTs take a more ad hoc approach to fitting the prediction rule (McBride and Nichols, 2018; Noriega-Campero et al., 2020).[7] Figure S5 lists the variables that yielded the largest coefficients in our Bangladesh data.

We additionally replicate some less common targeting approaches that are also relevant counterfactuals:

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final poverty prediction for each household is an average of the predictions from each decision tree.

[7] Using cross-validation, we evaluated several approaches to constructing a PMT, including simple linear regression, linear regression with step-wise forward selection, LASSO regression, and a random forest algorithm. When evaluated out-of-sample, we found that the LASSO regression was most accurate, so our main results focus on the LASSO PMT, where the L1 penalty is selected via cross-validation. In Appendix A.3, we show results for other PMT variants.

4. **Geographic targeting** at the union (admin-5) level, based on aggregating population-weighted wealth estimates from the global deprivation index (CIESIN, 2021), which combines subnational administrative datasets and gridded earth observation datasets to produce an index of relative deprivation. The components of the gridded GDI include the child dependency ratio, infant mortality rates, the subnational human development index, the remotely sensed ratio of built-up to non-built up area, nighttime lights intensity, and changes in nighttime lights intensity from 2012 to 2020. We aggregate the GDI at the union (admin-5) level, weighting by population using remotely sensed population data from Tiecke et al. (2017).

5. Other survey-based targeting approaches similar to the PMT, including Bangladesh's **poverty probability index (PPI)** and an **asset index** constructed with principal components analysis. The PPI is a scorecard poverty method based on 10 questions, including district, household members, children under ten, the highest grade completed by anyone in the household, ownership of a bicycle, refrigerator, and fan, construction material of household walls, electricity connection, and type of toilet used. The PPI scorecard was calibrated by Innovations for Poverty Action using the nationally representative 2016-17 Household Income and Expenditures Survey. Our asset index is constructed following Filmer and Pritchett (2001), using weighted principal components analysis to obtain a vector representing the direction of maximum variation in asset ownership among the 26 assets collected in our survey. In our setting, the first principal component explain on average 18% of the total variation in asset ownership.

6. **Peer rankings**, based on taking the average of the wealth ratings elicited in the household survey for a given household by their neighbors (see Appendix A.7). This is similar to the CBT in that it seeks to understand the extent to which neighbors correctly perceive each others' relative standing, but it obtains information from households individually and privately rather than through the collective and public process of the CBT. We ask households to also rate themselves, so the peer ranking module also

produces a "self-targeting" outcome. Unlike the CBT, these peer rankings were not incentivized and survey subjects were not told that their rankings would affect real transfers.

Data Ethics and Privacy Appendix A.8 provides details on the protocols we followed to minimize the risk of mis-use of call detail records (CDR). To summarize: we received permission from the Bangladesh Telecom Regulatory Commission to access CDR from the four major telecom operators in the country. We also secured informed consent from survey participants before accessing their CDR. To minimize the risk of data leaks and unauthorized use, the research team provided the telecom operator staff a set of phone numbers from the subset of surveyed households who granted us consent, along with code that would allow the telecom staff to extract the 1,578 features from the CDR data. Our research team never accessed the raw CDR. The telecom staff were responsible for merging a redacted version of the household surveys with the CDR features; this dataset was anonymized and securely stored on an isolated server on the premises of a2i, an entity of the Government of Bangladesh. This multi-step data handling protocol was designed to ensure that the research team, GiveDirectly, and the Bangladesh government never accessed the CDR with personal identifiable information (PII), and that the telecom operators never accessed the unencrypted household survey data.

3 Accuracy of targeting methods

Our first set of results compares the accuracy of the suite of targeting approaches enumerated in Section 2 for identifying the consumption-poorest households in our setting, with a particular focus on phone-based targeting (PBT), community-based targeting (CBT) and proxy-means testing (PMT). In this analysis, we use per capita household consumption expenditures, collected through our household survey, as the primary benchmark against which all targeting methods are evaluated.

Data from a randomly selected 75% of surveyed households are used to train

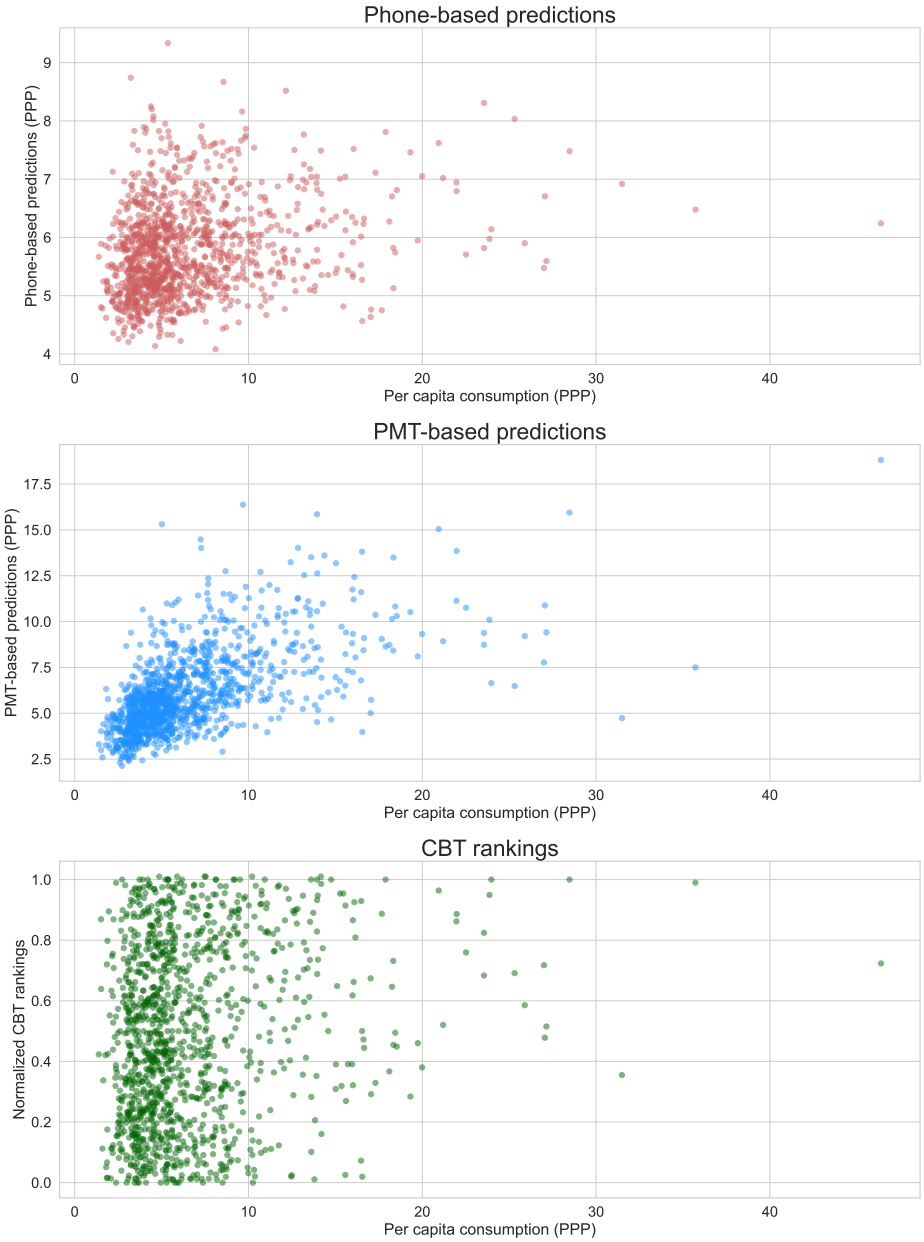
targeting methods that require machine learning (i.e., phone-based targeting and PMT), while the other 25% are used for the evaluation. We repeat this process 100 times on different random train-test splits, and report the mean and standard deviation of each accuracy metric over the 100 runs.[8] To illustrate, Figure 1 shows scatterplots from one train-test split of the rankings under each method vs. per-capita consumption expenditure as measured in the household survey. Our results on relative accuracy below can be anticipated by noting that PMT (center) produces the tightest distribution, followed by PBT (left) and then CBT (right).

Accuracy metrics We use three standard metrics for assessing targeting methods. The first and most intuitive is *recall*: the probability that a truly poor household will be correctly classified as poor.[9] This is the simplest metric, but considers only binary errors, not the magnitude of error, and depends on the specific threshold of a particular program. The second metric is the *Spearman rank correlation* between the rank assigned to a household by a particular method and the household's true rank in the distribution of consumption per capita. This puts less weight on the exact classification of households near the cutoff, and penalizes large errors in ranking households. The third is the Area under the ROC (receiver operating characteristic) curve, or *AUC*, which summarizes targeting accuracy not just at a single classification threshold (in our case, the 21% quota), but rather for all possible classification thresholds

[8]Some of the targeting methods we simulate do not produce poverty rankings for all households. For instance, in the phone-based targeting approach, 6% of households are not given a wealth ranking (2% of households in the survey do not provide a phone number or do not consent to matching survey data to mobile phone records; 4% of households in the survey provide at least one mobile phone number but no number is associated with transactions that appear in our mobile phone metadata). 0.4% of households were not ranked in the CBT exercises and 2% of households had no peer rankings because they were not known to the community. In such cases, households that are unranked are targeted last in our targeting simulations – that is, we assume that any household without a ranking is prioritized for aid after all households with rankings.

[9]Recall, also known as *sensitivity*, is equal to one minus the type II error rate. Since the program provided transfers to a fixed number of beneficiaries (the 21% quota), *recall* and *precision* – which is the share of households classified as poor that are truly poor (one minus the type I error rate) – are equal in our setting.

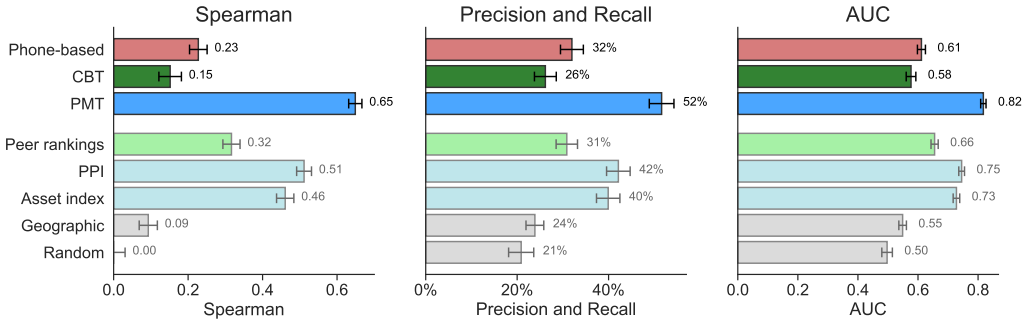
Figure 1: Predictions vs. survey-based household PCE, by method



Notes: these figures show scatterplots of per-capita consumption predicted from PBT (left) and PMT (center), and rankings from CBT (right) vs. household PCE per capita as measured in the household survey. Produced using one train-test split.

(i.e., quotas that range from 0% to 100%).[10] Accurate classifiers yield high true positives and low false positives for a variety of classification thresholds. A perfect classifier achieves an AUC value of 1, whereas a random classifier (that targets randomly chosen households to fill the quota) achieves a value of 0.5.[11]

Figure 2: Comparing accuracies of targeting methods



Notes: these figures compare the accuracy of our targeting mehods, based on Spearman correlation with consumption (left), precision and recall for identifying the 21% consumption-poorest households (middle), and area under the ROC curve (right). Error bars show two standard deviations above and below the mean for each metric.

Main results on targeting accuracy Figure 2 reports targeting accuracies for each of the targeting methods we evaluate. We observe that phone-based

[10] Specifically, the ROC curve shows how the true positive rate (recall) varies as a function of the false positive rate, for each possible classification threshold between 0 and 1. When the threshold for being classified as poor is low (e.g., if benefits are provided to any household that has more than a 5% chance of being poor), most households are targeted (resulting in high true positives, but also high false positives); by contrast, when the threshold is high, few households will be targeted (low true positives, low false positives).

[11] All of these accuracy metrics are designed to evaluate each method's ability to identify low-consumption households. For the PMT, PPI, and phone-based targeting, where machine learning models are trained to predict consumption, this is a natural evaluation. However, it was not practically feasible in the CBT or peer-ranking exercise to ask households about the consumption of other households. Instead, the CBT protocol followed standard practice and asked community members to identify community members with the lowest "socio-economic status", and the peer rankings asked households to identify the households that "have the least." To explore these nuances, we look more closely to see what types of households each method targeted (beyond the consumption-poor), after presenting the main results on targeting accuracy.

targeting (AUC = 0.61; precision/recall = 32%) is more accurate at identifying the consumption-poor than CBT (AUC = 0.58; precision/recall = 26%). However, both approaches are substantially less accurate than PMT (AUC = 0.82; precision/recall = 52%). The differences between the three methods are statistically significant ($p < 0.001$, using a Wilcoxon signed-rank test). Other survey-based targeting variants (AUC = 0.73-0.75; precision/recall = 40-42%) also outperform phone-based targeting and CBT but are worse than the PMT.

The decentralized peer ranking approach outperforms the CBT and is comparable in accuracy to phone-based targeting (AUC = 0.66; precision/recall = 31%). Table 1 provides comprehensive targeting accuracy metrics for these targeting methods, as well as a few other variants described in Appendix A. When we limit attention only to people's self-ratings in the peer-rankings exercise, we observe reasonable accuracy comparable to phone-based targeting or using all peer ratings. But self-ratings are most susceptible to strategic behavior and 'gaming' by beneficiaries.

Binary classification errors do not capture potential differences in *magnitudes* of errors. That is, two classification methods could have similar error rates for a given threshold, but a method with "small" mistakes (tending to exclude households just below the threshold and include households just above the threshold) is likely to be preferred to a method with "larger" mistakes (tending to exclude households far below the threshold and include households far above the threshold). In Figure S4, we assess magnitudes of errors by showing the distribution of consumption per capita for households included and excluded by each targeting approach. Figure S4 suggests that the PMT tends to include poorer households than phone-based targeting and CBT, and that phone-based targeting includes poorer households than CBT. Similarly, the households excluded by PMT are on average richer than the households excluded by phone-based targeting, which are in turn richer than the average household excluded by CBT.

Who is targeted by each method? Figure S5 highlights the variables selected by the PMT. These include demographic characteristics (large house-

Table 1: Accuracy metrics for all targeting method variants

Targeting Method	Spearman	Precision	AUC
Panel A: Main targeting options			
Phone-based targeting	0.23 (0.02)	32% (3%)	0.61 (0.01)
CBT	0.15 (0.03)	26% (2%)	0.58 (0.02)
PMT (LASSO)	0.65 (0.02)	52% (3%)	0.82 (0.01)
Random	0.00 (0.03)	21% (3%)	0.50 (0.02)
Panel B: PMT variants			
PMT (OLS)	0.65 (0.02)	51% (3%)	0.82 (0.01)
PMT (Stepwise)	0.64 (0.02)	51% (3%)	0.81 (0.01)
PMT (Random Forest)	0.62 (0.02)	48% (3%)	0.80 (0.01)
Panel C: Other Survey-based targeting options			
PPI	0.51 (0.02)	42% (3%)	0.75 (0.01)
Asset index	0.46 (0.02)	40% (3%)	0.73 (0.01)
Panel D: Geographic targeting options			
Unions	0.09 (0.02)	24% (2%)	0.55 (0.01)
Villages	0.09 (0.03)	24% (2%)	0.54 (0.01)
Neighborhoods	0.08 (0.03)	24% (3%)	0.54 (0.01)
Panel E: Decentralized CBT			
All ratings	0.32 (0.02)	31% (2%)	0.66 (0.01)
Neighbor ratings only	0.23 (0.02)	28% (3%)	0.61 (0.01)
High confidence neighbor ratings only	0.32 (0.02)	31% (2%)	0.66 (0.01)
Own rating only	0.40 (0.02)	30% (1%)	0.67 (0.01)
All rankings	0.15 (0.03)	25% (3%)	0.57 (0.02)
Neighbor rankings only	0.03 (0.03)	22% (2%)	0.52 (0.02)
High confidence neighbor rankings only	0.09 (0.03)	25% (3%)	0.55 (0.02)

Notes: Comparison of targeting accuracy metrics for all targeting variants described in Appendix A. Standard deviations across 100 bootstrap simulations are shown in parentheses.

holds with lots of children, disabled household head), information on asset ownership (those lacking vehicles, fridges, large plots of residential and agricultural land, and large houses with cement roofs), as well as the household's geographic location. [12] For comparison, Table S2 shows the features of mobile phone use that are most correlated with per-capita consumption. These include "recharge behavior", which indicates how much money the subscriber adds to their SIM card each time they buy phone credit,[13] how frequently they use mobile data (which might be a proxy for owning a smartphone), features of their network such as the number of unique phone numbers the user connects to for incoming or outgoing calls, and aspects of their mobility as inferred from the location of cell towers with which the phone connects.

Table S3 presents multivariate regressions that identify the household and community-level characteristics that are predictive of inclusion for the various targeting methods we study: phone-based targeting, community-based targeting, PMT, and decentralized peer rankings. Most notably, both community-based targeting and decentralized peer rankings are more likely to select widows/widowers for transfers than either the PMT or PBT — a result that is consistent with the community-based targeting in Indonesia studied by Sumarto et al. (2025). As in Indonesia, community members may be making use of local, private information about the idiosyncratic disadvantages faced by specific households, which may not get reflected in surveys or in patterns of phone use. Both phone-based targeting and the PMT are better at identifying households that spend a large share of their budget on food (a proxy for the household's subsistence risk - see Bryan et al. (2014)), although this variable is a positive predictor under all methods.

Table S3 also identifies some of the biases inherent in phone-based targeting.

[12] Note that, while 5 of the top 20 PMT variables are geographic indicators (unions), purely geographic targeting performs poorly, as seen in, e.g., Figure 2. This suggests that, to the extent that location contains useful information, the LASSO estimation used in the PMT will capture it, but using location exclusively will overlook large variation within geographic units.

[13] In Bangladesh, the vast majority of subscribers are on prepaid contracts. For these phones, the subscriber has to first add value to their account via recharge, and can then use the available balance on their account to make calls, send text messages, and so forth.

Phone ownership is curiously a *positive* predictor of selection, since households without phones were mechanically excluded by our phone-based selection process. However, conditional on ownership, both PBT and PMT exclude households with more frequent phone usage (those with larger number of calls and messages) – which may be a hidden proxy for deprivation that community targeting fails to pick up on. At the neighborhood level, the PMT targets more unequal communities with lower average consumption levels. Phone-based targeting directs transfers to households with fewer social connections; this suggests that the phone data may help reveal the extent to which households are socially isolated.[14] At the neighborhood level, the PMT targets more unequal communities with lower average consumption levels. None of the targeting strategies disproportionately favor or disfavor minority households or minority-dominated neighborhoods.

Heterogeneity: Do some methods perform better on specific types of households or neighborhoods? While our results thus far indicate that PMT targeting is substantially more accurate than the other options, and that phone-based performs better than community-based targeting, the aggregate results may mask important heterogeneity. For instance, CBTs might work better in more homogenous neighborhoods, or PBTs might work best with active phone users. However, we find little evidence that the relative performance of different targeting methods varies systematically by neighborhoods or household type. In Panel A of Figure S6, we observe that the PMT generally performs better than phone-based, which performs better than CBT, across all different types of communities — including when disaggregating by community size, by share of non-Muslim or non-Bengali minority households, etc. Panel B of Figure S6 tells a similar story with respect to heterogeneity by household characteristics (household size, household head gender/employment/minority

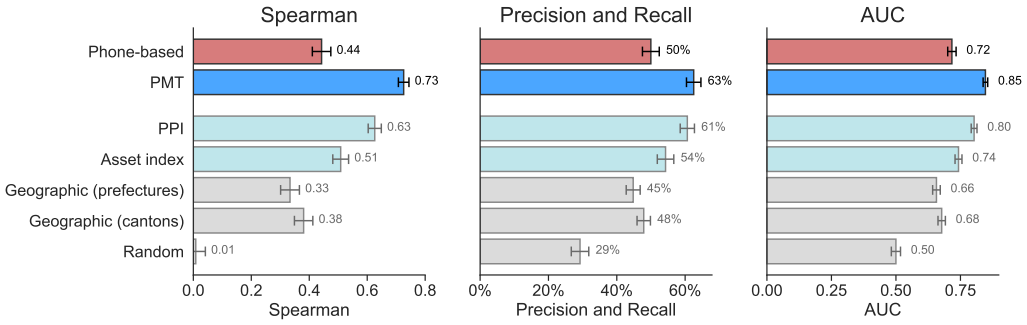
[14] In the peer rankings module, each household was asked, for eight randomly selected households in their neighborhood, how well they know the household on a scale of 1-4. Connectedness at the neighborhood level is defined as the average knowledge ranking for all households in the neighborhood. A household's "connectedness" is defined as the average knowledge ranking others assign to that household.

status, connectedness, and amount of phone use (measured as the total number of calls and texts placed over the study period). Across all types, PMT performs best, and phone-based generally beats CBT , for all types of households and neighborhoods. Phone-based and CBT are statistically comparable, but phone-based targeting almost always outperforms CBT, except within the top quartile of household size.

Figure S6 also allows us to examine the absolute (as opposed to relative) performance of each targeting method across neighborhood and household type. Community-based targeting works better in more urban neighborhoods, and where average poverty levels are high. Interestingly, there is little variation in CBT performance by the minority share, size, and neighborhood connectedness. Both PBT and CBT are a bit more accurate *within* the set of non-minority households.

Targeting *within* Neighborhoods The analysis presented thus far compares targeting methods in terms of how accurately each method identifies the poorest households from the overall study sample, which matches the goals of the GiveDirectly program we implemented using CBT. However, some programs may seek to identify the poorest households within each community, with a quota assigned at the community level. Importantly, in the CBT approach we implement, communities were asked to rank households from poorest to richest, and were told that the poorest 20% of households within each community would receive a transfer. It is therefore possible that – while the CBT is weaker than phone-based targeting overall – it is better at identifying the poorest share of households within each community. To assess this possibility, we repeat the targeting evaluation with the objective of identifying the poorest 21% of households *within each neighborhood*. In Figure S7, we show that while the absolute accuracy of each targeting method declines with this evaluation approach (this is unsurprising, since geographic variation between communities is no longer a useful signal for targeting), the quality of targeting approaches relative to one another is unchanged: phone-based targeting is still more accurate than CBT, and less accurate than PMT.

Figure 3: Targeting accuracy in Togo study



Notes: this figure shows the accuracy of different targeting methods for the country of Togo, reproducing results in Aiken et al. (2022). As in our analysis in Bangladesh in Figure 2, accuracy is calculated over 100 random train-test splits, and error bars show two standard deviations above and below the mean for each metric. This bootstrapping procedure explains very slight differences to the results presented in Aiken et al. (2022), where 1,000 train-test splits were used.

Generalizability The performance of phone-based targeting in Bangladesh is broadly consistent with what prior work has found evaluating a similar set of targeting approaches in Togo. In Figure 3, we replicate the results of Figure 2, instead using data from Togo (Aiken et al., 2022). In both settings, we find that the PMT is substantially more accurate than phone-based targeting. However, the gap between phone-based targeting and PMT is wider in Bangladesh (63% difference in precision and recall and 34% difference in AUC) than in Togo (26% difference in precision and recall and 18% difference in AUC). The previous work in Togo did not include CBT as a possible targeting approach.[15]

More generally, across all targeting methods, targeting accuracy is relatively low in our setting (AUC = 0.52-0.82; precision and recall of 23-52%). We compare our results to three other published targeting evaluations (which primarily focus on PMT and CBT) to see whether this is unusual: (1)Aiken

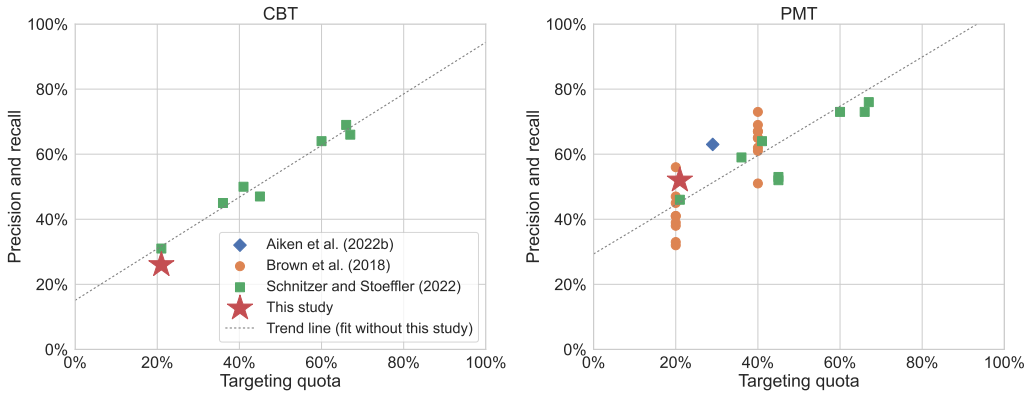
[15]Our finding that PMT is also more accurate than CBT is consistent with most other papers that have compared the two methods (Schnitzer and Stoeffler, 2022; Premand and Schnitzer, 2021; Alatas et al., 2012). However, the difference in our setting is relatively more extreme: we find that switching from CBT to PMT doubles precision and recall (from 26% to 52%) and increases AUC by 41% (from 0.58 to 0.82).

et al. (2022), which calculates targeting accuracy nationwide in Togo for a PMT with a 29% targeting quota; (2) Schnitzer and Stoeffler (2022), which evaluates the targeting accuracy of seven CBT-based and eight PMT-based social protection programs run in parts of Burkina Faso, Cameroon, Mali, Niger, and Senegal with targeting quotas ranging from 21% to 67%, and (3) Brown et al. (2018), which simulates PMT-based country-level targeting in eight African countries with 20% and 40% targeting quotas. Figure 4 plots the precision and recall of CBT and PMT in each of these studies as a function of the targeting quota used. It shows that targeting accuracy under both methods increase linearly with the size of targeting quota. The fit is remarkably tight despite large variations in data, program implementation, and study contexts. Importantly, our results appear to be well within the range of results reported in past work.

Another possible reason for the low targeting accuracy in our setting is the narrow geographic scope of the program we study. Our study is limited to 180 neighborhoods in Cox's Bazar district. As a result, there is likely substantially less variation in poverty in our setting than in the settings of national-scale social protection programs. To test this hypothesis, Figure S8 simulates targeting more homogeneous subsets of our study population by poverty. The results confirm that, for all methods except for random and geographic targeting, targeting simulations that are restricted to poorer subsets of the households in our survey result in lower targeting performance than evaluations conducted with the full set of households in our survey.

Combining targeting methods It is possible that the targeting data sources could complement each another, such that a combined approach improves overall targeting accuracy. Figure S9 shows the results of a simple strategy for combining targeting approaches. Our algorithm for augmenting method A with targeting method B is to replace the very last household deemed worthy of a transfer under method A with the poorest household identified under method B who was excluded under method A. Such replacements can be repeated until all method-A-targeted households are replaced with method-B-targeted

Figure 4: Comparison of targeting accuracy with other studies



Notes: these figures show comparisons of our results on targeting accuracy (red stars) with past studies that also use a quota approach to targeting evaluation (green squares for Schnitzer and Stoeffler (2022), blue diamonds for Brown et al. (2018), and orange dots for Aiken et al. (2022). The targeting error rate is shown as a function of the targeting quota.

households. This yields a continuum of A-B combined targeting, where the "mixing parameter" (share of A-targeted households replaced with B-targeted households) varies from 0% to 100%. Figure S9 shows that combining rankings using this method does not improve overall targeting accuracy. Neither the phone + PMT nor the CBT + PMT approaches improve precision and recall relative to solely using the PMT rankings. We find that adding decentralized peer rankings can improve pure phone-based targeting by a few percentage points, but that the combination of phone + CBT does not improve over pure phone-based targeting.

An alternative approach to combining targeting data sources is to include variables from multiple data sources in the ML models used to train the PMT and phone-based targeting methods.[16] Figure S10 shows that the ML-based approach to combining data sources also does not substantially improve accuracy relative to using single data sources individually.

[16]Specifically, for the phone + PMT-based approach, all phone features and PMT features are included in the model. For the phone + CBT-based approach, phone features and the CBT rankings are included in the model. For the PMT + CBT-based approach, PMT features and the CBT rankings are included in the model.

4 Cost-effectiveness results

Thus far, our results suggest that proxy-means testing is more accurate than phone-based targeting, and that phone-based targeting is more accurate the community-based targeting. However, the costs associated with these different targeting methods also vary substantially. For instance, phone-based targeting is typically much cheaper than proxy-means testing – especially for large scale programs – because phone-based targeting does not require in-person primary data collection for screening. This creates a trade-off between cost and accuracy. In this section, we use a simple framework to characterize the conditions under which the different targeting methods would be more "cost-effective" Hanna and Olken (2018).

We assume that the implementer has a total budget B and chooses between targeting methods to identify and send as much money as possible to the neediest individuals. Method m incurs targeting costs C_m, leaving $T_m = B - C_m$ for transfers. We assume that the implementer wishes to target I "included" households out of a total population of $I + E$ (included and excluded) households, and is constrained to make equal payments b_m to each recipient, with $b_m = (B - C_m)/I$.[17] We assume a household constant relative risk aversion (CRRA) utility function, so household i's utility is given by

$$U_i = \frac{c_i^{1-\sigma}}{1-\sigma},$$

where consumption c_i is equal to the household's pre-program consumption level y_i plus the transfer if the household receives it, so $c_i = y_i + 1\{i \in I\} b$. We assume that the implementer has an objective function that maximizes the

[17] We take the total budget B and the number of people targeted I as given, although in principle either or both could depend on the accuracy of the targeting method. We constrain the implementer to equal transfers for simplicity, which reflects most real-world social protection programs.

unweighted sum of household utilities

$$V = \frac{1}{1-\sigma}\sum_{i=1}^{N} c_i^{1-\sigma}$$
$$= \frac{1}{1-\sigma}\sum_{i\in I}(y_i + b)^{1-\sigma} + \frac{1}{1-\sigma}\sum_{i\in E} y_i^{1-\sigma}.$$

Due to diminishing marginal utility, the implementer prefers to allocate transfers to households with lower pre-program y_i, but faces a tradeoff if identifying and targeting such households is more costly (and therefore reduces b).

After fixing a hypothetical program's budget and the number of people screened, we calculate the screening costs associated with different targeting approaches, and then calculate the total budget remaining that can be provided as benefit transfers. Fixing the targeting threshold at 21% — as in GiveDirectly's program in Bangladesh — we then allocate the transfers to the targeted households and calculate V under each possible targeting regime. Following Hanna and Olken (2018), we use $\sigma = 3$ to calculate V. We first calculate V_0, the value of the implementer's objective function in the absence of the program, and V_{1B}, the "first-best" value, i.e., if the implementer could costlessly obtain the exact ranking of all households and target perfectly. The gain in this first-best scenario, then, is $V_{1B} - V_0$. We then calculate V_m, the value of the objective function for each method m (i.e., PMT, PBT, CBT), and report G_m, the gain relative to the first-best:

$$\text{Relative gain from method } m = G_m = \frac{V_m - V_0}{V_{1B} - V_0}. \tag{1}$$

Data on Costs. Screening costs are a key input for computing G_m. To analyze cost-effectiveness, we therefore supplement our survey data and mobile phone records with detailed information on the costs of administering each targeting approach. Both the PMT and phone-based targeting approaches require a detailed household consumption survey for benchmarking and cali-

bration. In Bangladesh, this cost $46,600 for 5,000 households.[18] We estimate the cost of the PMT using costs from our census, which lasted approximately 15 minutes (similar to the time required for a typical PMT scorecard) and cost approximately $1.25 per household, in addition to fixed costs of $6,300 for enumerator training and equipment. This marginal cost per household for a PMT is lower than typical: past work that reviewed the published PMT costs in the research literature found that the median reported PMT cost is $4.00 (Aiken et al., 2023c). Phone-based targeting incurs a fixed cost for researcher time in implementing the machine learning method, but the marginal cost per household is approximately zero. Our CBT exercises had a variable cost of $2.33 per household screened, plus a fixed cost for training and equipment of $19,300. Because CBT is both more expensive *and* less accurate than phone-based targeting in our setting, we focus primarily on comparisons between PMT and phone-based targeting.[19]

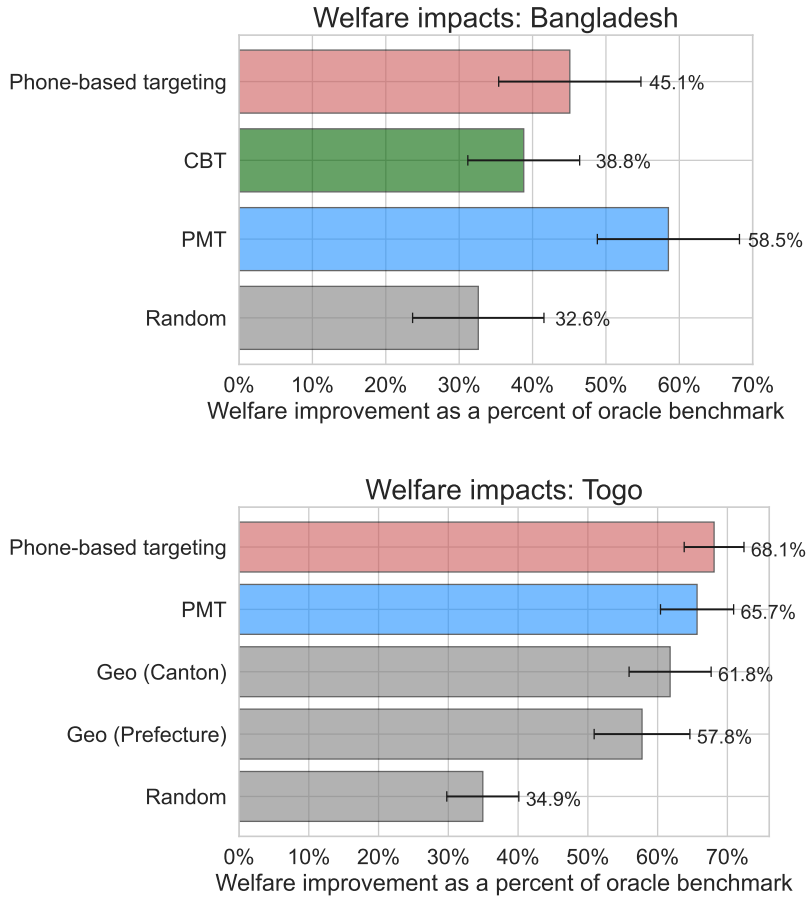
Cost-effectiveness Results for Bangladesh We begin by computing G_m generated by the GiveDirectly program in Bangladesh (described in Section 2), which had a budget of roughly $5 million and screened around 100,000 households. Based on our own surveys, we estimate the total screening costs for a PMT in this setting at $177,900, leaving around $4.8 million for cash transfers. The screening costs for phone-based targeting were $46,600, leaving around $4.95 million for cash transfers.

Figure 5 (top panel) shows the gains from the GiveDirectly transfers in southern Bangladesh under the assumption of CRRA utility, relative to the gains generated by costless perfect targeting (V_{1B}). Despite the higher costs of the PMT, the higher targeting accuracy of the PMT results in larger gains than phone-based targeting (58.5% of the gains of costless perfect targeting vs.

[18]The consumption survey cost of roughly $10 per household in our setting is much lower than other costs reported in the literature: Kilic et al. (2017) report costs ranging from around $50-500 per household surveyed ($200,000 to over $4 million total) for nationally representative consumption surveys enumerated as part of the Living Standards Measurement Surveys program.

[19]Our CBT cost is similar to other costs reported in the literature, which Aiken et al. (2023c) report has a median per-household CBT cost of $2.20.

Figure 5: Gains by targeting method



Notes: These figures show the gains in the implementer's objective function by method. As in Equation 1, gains are relative to the gains that could be obtained under perfect information. These figures are based on the design and parameters of the cash transfer programs implemented in Bangladesh (top panel) and Togo (bottom panel) (see Section 4). The Bangladesh program had a $5 million budget for 100,000 households screened, targeting 21% of households, with targeting accuracy shown in Table 1. The Togo program had a $5 million budget for 207,000 households screened, targeting 29% of households, with targeting accuracy shown in Figure 3.

45.1%). The CBT approach, which is both more costly and less accurate than phone-based targeting, produces smaller gains (38.8%).

Comparison to Togo To understand the generalizability of our findings, we repeat this calculation for the GiveDirectly-Novissi (GD-Novissi) program in Togo described in Aiken et al. (2022). GD-Novissi also had a budget of roughly $5 million, and screened roughly 207,000 households. GD-Novissi, like GiveDirectly's program in Bangladesh, targeted transfers using mobile phone metadata. For our analysis of cost-effectiveness of GD-Novissi, we use nationally representative survey data from Togo collected in 2018, matched to mobile phone records from the same year.

Several key differences between the two research settings in Bangladesh and Togo are worth noting: First, nearly twice as many households were screened in Togo, though the program had roughly the same total budget as the Bangladesh program. Further, as we saw in Table 1 and Figure 3, phone-based targeting in Togo was substantially more accurate (much closer to PMT) than in Bangladesh. This was likely due to the fact that (a) the survey in Togo was nationally-representative, whereas in Bangladesh we focus on three sub-districts, resulting in a study population with substantially less geographic and socioeconomic heterogeneity; and (b) the survey in Togo was restricted to households that provided a phone number that could be matched to mobile phone metadata, so non-phone-owning or unmatched households are not included in the analysis. In contrast, households without phones are included in the Bangladesh analysis and assumed to be targeted *last* under the phone-based targeting approach. Finally, no community-based targeting data was collected in Togo, so we can only compare PMT and phone-based targeting there.

The bottom panel of Figure 5 repeats the same calculation as in Bangladesh for the GD-Novissi program in Togo. Screening costs for the PMT in Togo would be $258,750, leaving around $4.75 million for cash transfers. The larger screening costs for the PMT and the better accuracy of phone-based targeting in Togo jointly result in gains from phone-based targeting (68.1% of the gains from

costless perfect targeting) that exceed the gains produced by PMT (65.7%).

These contrasting results from Bangladesh and Togo illustrate how the relative cost-effectiveness of phone-based targeting and proxy-means testing depends on both the scale of the aid program (in terms of budget and screening costs) as well as the relative accuracy of the targeting methods being compared. Our next set of results analyzes this tradeoff more systematically by varying the scale and scope of several hypothetical transfer programs.

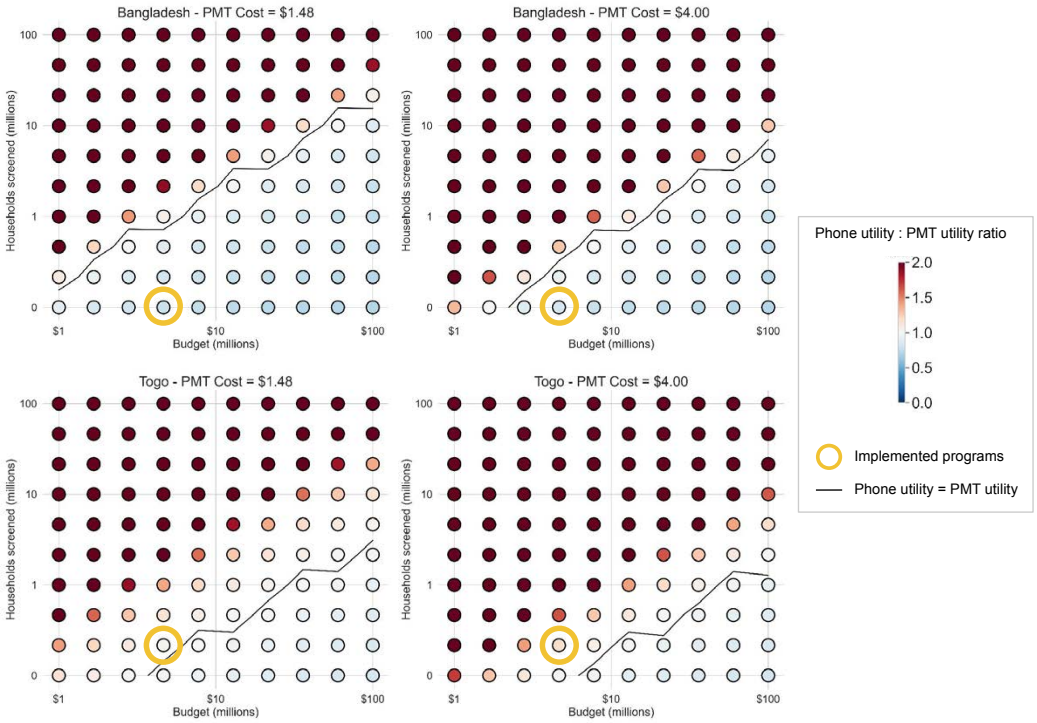
Comparing cost-effectiveness as a function of program scale The GiveDirectly programs we analyze are fairly small scale — both in terms of the total budget and the number of households screened for eligibility — relative to national-scale social protection programs typically run by governments. For example, government cash transfer programs in Bangladesh typically have budgets of $10-300 million, and a mandate to screen all 41 million households in the country for eligibility.[20]

Figure 6 provides a visual comparison of the performance of phone-based targeting and PMTs for a range of hypothetical programs, varying both the total program budget (horizontally) and the number of households screened (vertically). When transfer programs have a large budget relative to the size of the population screened, like GiveDirectly's programs in Bangladesh and Togo, then PMT-based targeting results in larger gains. However, for programs with small budgets relative to the number of households screened, which characterizes many real-world government-run social protection programs in Bangladesh and elsewhere, phone-based targeting is preferred. This is mainly because the marginal cost of screening additional beneficiaries using mobile phone meta-data – once an algorithm is already developed – is essentially zero.

The top-left panel of Figure 6 roughly corresponds to the cost structure of the GiveDirectly program in Bangladesh. The circled point illustrates that for the actual program that was implemented in Bangladesh, the PMT outperformed phone-based targeting in terms of V. This occurs partly because

[20]These figures are taken from the budgets of large cash assistance programs in the fiscal year 2019-2020, reported in World Bank (2021).

Figure 6: Cost-effectiveness of PMT vs. PBT as parameters vary



Notes: Figures depict ratios of gains between phone-based targeting (PBT) and proxy means testing (PMT) as a function of a hypothetical social protection program's budget (x-axis) and households screened (y-axis). Red shades (darker) represent program scales at which phone-based targeting is preferred, blue shades (lighter) represent program scales at which PMT is preferred, and the line identifies the "decision threshold". Left: PMT variable cost of $1.48 per household screened, based on the costs of our surveys in Bangladesh. Right: PMT variable cost of $4.85 per household screened, based on the median of values reported in the literature. Above: Using data from Bangladesh. Below: Using data from Togo.

the variable per-household screening cost for the PMT in Bangladesh ($1.48) was unusually low, reflecting uniquely low costs of data collection in Bangladesh (Aiken et al., 2023c). The right two panels of Figure 6 show how the relative performance of PMT changes if the cost of screening households in Bangladesh were in line with the median per-household screening cost reported in the literature of $4.00 (Aiken et al., 2023c). For more typical PMT screening costs, the scope and scale of programs where phone-based targeting is preferred to PMT expand.[21] More broadly, Figure 6 highlights how a key factor in determining which targeting method performs best is the ratio of the program budget to the number of households screened. Phone-based targeting looks relatively more attractive for national-scale programs that attempt to screen a large number of individuals to make smaller per-household transfers.

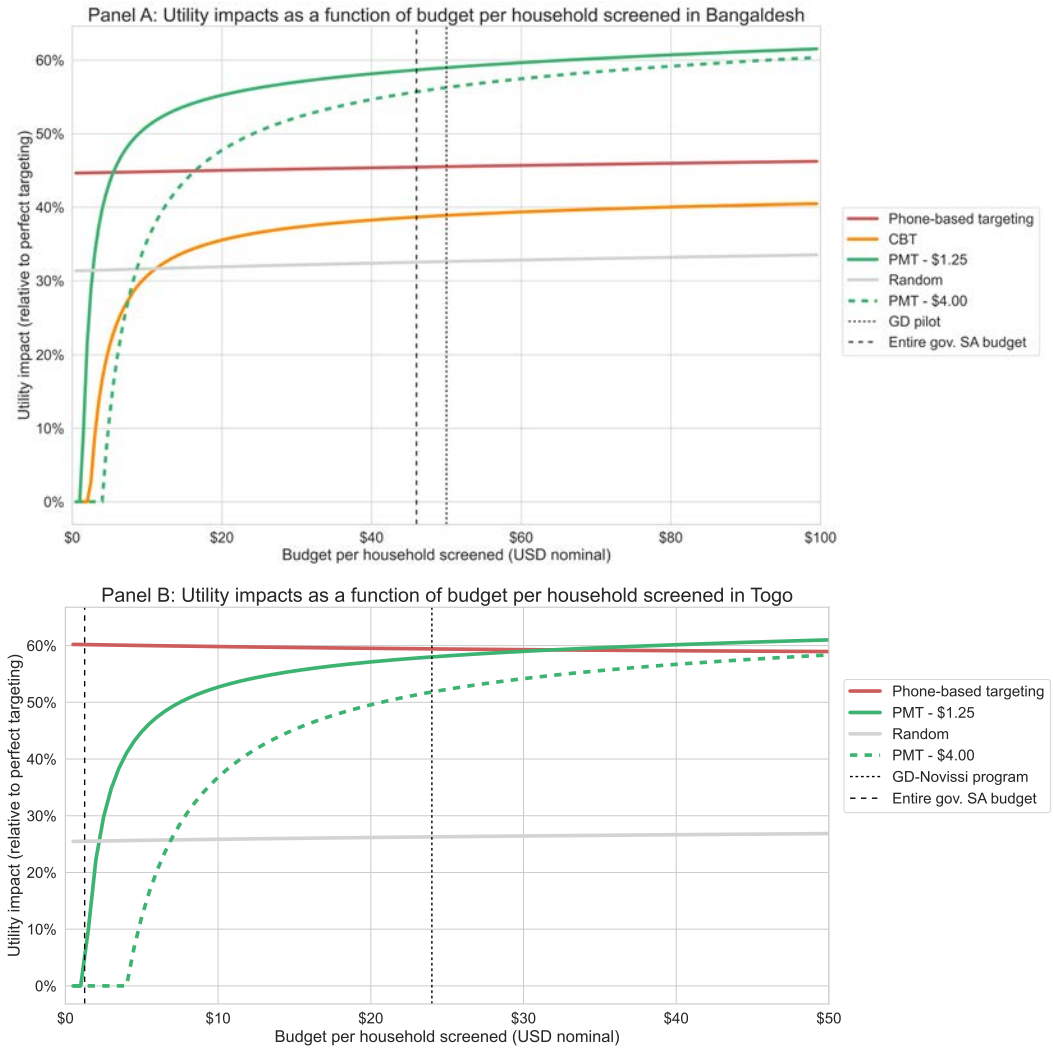
Figure 7 illustrates the thresholds at which different targeting methods provide the largest increases in the implementer's objective function, as a function of the *program budget per household screened*.[22] In Bangladesh (Panel A), phone-based targeting (red line) is preferred to the PMT for programs with budgets under $4 per household screened if the PMT costs $1.25 (solid green line); however, if the PMT costs the "industry-standard" $4.00 (dashed green line), phone-based targeting is preferred for budgets up to $15 per household screened. In Togo (Panel B), phone-based targeting is preferred for a wider range of program budgets: when the PMT variable cost of $1.25 is used, phone-based targeting is preferred for programs with budgets under $31 per household screened; with the more typical PMT cost of $4.00, phone-based targeting is preferred for budgets under $51 per household screened.

To anchor these comparisons to real-world social protection program scenarios, Figure 8 plots budgets as a function of the number of households screened for a number of countries (across the GDP per capita spectrum) using data

[21] Figure S11 further illustrates how the performance of PMTs and phone-based targeting vary with other important aspects of program design, including the fraction of beneficiaries targeted, the coefficient of relative risk aversion, and the variable cost of the PMT.

[22] In constructing Figure 7, we ignore fixed costs. For medium- to large-scale programs, these will be a tiny fraction of total costs; for example, fixed costs make up 4% of screening costs for a PMT-targeted program screening 1 million households, but only 0.4% of screening costs for a PMT-targeted program screening 10 million households.

Figure 7: Gains by method as a function of budget per household screened



Notes: These figures show gains in the implementer's objective function as a function of the program's budget per household screened. As in Equation 1, gains are relative to what could be achieved with costless perfect information. The top panel uses our data from Bangladesh. This analysis requires fixed costs to be dropped from calculations, implicitly assuming that fixed costs are negligible when the number of households screened are sufficiently large. The solid green line uses the PMT variable cost of $1.48 per household screened, based on the costs of our surveys in Bangladesh, and the dashed green line the value of $4.85 per household screened, based on the median of values reported in the literature. The bottom panel uses data from Togo. In each figure, the dashed vertical lines show the budget of the aid program by GiveDirectly and the entire government cash assistance budget.

from the World Bank's ASPIRE database. The figure shows where existing programs fall relative to the two thresholds of $51 per household (using costs and accuracy from Togo) and $15 per household (using costs and accuracy from Bangladesh). Sixty-six of 95 countries have budgets over $51/hh, and so PMT would be preferable under both thresholds; 10 of 95 countries have budgets that are sufficiently low (less than $15/hh) to prefer phone-based targeting under both thresholds; and 19 of 95 are intermediate cases, with PMT preferred using cost and accuracy estimates from Bangladesh but phone-based targeting preferred using cost and accuracy estimates from Togo.[23]

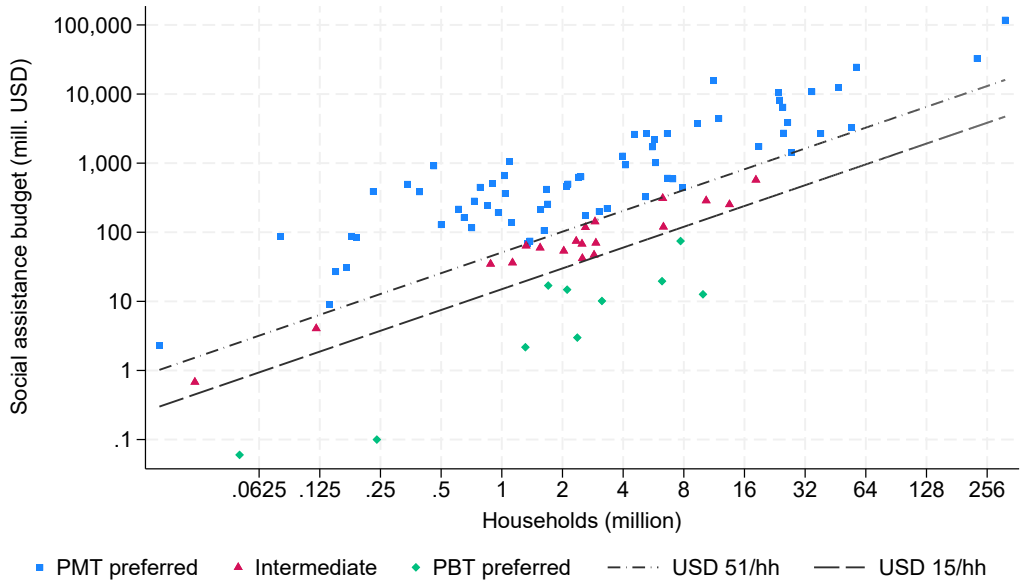
Sensitivity of cost-effectiveness results The analysis in this section has emphasized how the choice of the preferred targeting method (i.e., the one that maximizes G_m) depends on the size of the program relative to the number of households screened. Here, we briefly illustrate how this determination is influenced by other aspects of the program design and assumptions about the shape of the utility function.

Perhaps most notably (and intuitively), the results in Figure 7 are quite sensitive to the accuracy of the underlying targeting technology. In comparing Panels A and B of Figure 7, we saw that in Togo, where phone-based targeting is more accurate, phone-based targeting was preferred for a larger range of programs (i.e., Figure 7). Figure S12 shows how the gains from phone-based targeting increase as the targeting algorithm becomes more accurate (see Appendix B for details on these simulations). In both Bangladesh (Panel A) and Togo (Panel B), when the Spearman correlation between phone-based poverty estimates and consumption is around 0.20 (as in Bangladesh), programs with budgets under $15 per household screened should use phone-based targeting. As the correlation increases to 0.40 (as in Togo), phone-based targeting performs better for programs up to $40 per household screened. The exact points at which phone-based targeting would be preferred over a PMT are shown in Table S4.

Figure S11 illustrates how three other factors likewise influence the perfor-

[23]We provide country-by-country details in Appendix E.

Figure 8: Social assistance: total budgets vs households screened



Notes: this figure plots countries' budgets for cash-based social assistance transfers (y-axis) versus the number of households to be screened (x-axis). Each of the 95 points represents a country. The upper dashed diagonal line denotes a budget of $51 per household screened, which is the cutoff between cost-effectiveness of PMT (above) and phone-based targeting (below) based on screening costs and accuracy from the Togo study. The lower dashed diagonal line denotes the PMT vs. phone-based cutoff of $15 per household screened using parameters from the Bangladesh study. For programs above the upper line (blue squares, 66 observations), PMT is preferred to phone-based targeting under both scenarios. For programs below the lower line (green diamonds, 10 observations), phone-based targeting is preferred to PMT in both scenarios. For programs in between the two (red triangles, 19 observations), PMT is preferred to phone-based targeting using parameters from Bangladesh, but phone-based targeting is preferred to PMT using parameters from Togo. Data are from the World Bank's Aspire database, World Bank Open Data and the Global Data Lab, and are described in greater detail in the main text.

mance of phone-based targeting relative to the PMT. In the left panels, we observe how screening costs affect the relative gains (G_m) of each targeting method: as the variable cost of the PMT increases, the gains from PMT decrease. In Togo, phone-based targeting is more cost-effective for almost all PMT costs (bottom-left panel); in Bangladesh (top-left panel), phone-based targeting is only preferred when PMT costs exceed roughly $14 per household (for context, the highest PMT cost reported in the survey by Aiken et al. (2023c) is $9.50). The middle panels illustrate the sensitivity of our results to the coefficient of relative risk aversion σ: when σ is small (i.e., the cost of targeting errors are relatively small), the differences between methods are smaller, while at very high values of σ gaps increase. In Togo (bottom-middle panel), we observe that when σ is very high, and consumption of the most poor matters more, the PMT becomes preferred: despite its high cost relative to PBT, the additional accuracy becomes more valuable, because as σ increases the cost of errors becomes greater. Finally, the right panels of Figure S11 show that the rankings of methods are not particularly sensitive to the targeting threshold. In Bangladesh, PMT uniformly outperforms PBT, while PBT outperforms CBT. In Togo, PMT outperforms PBT up to a threshold of approximately 25%, while the two perform similarly at higher thresholds.

5 Discussion and Conclusion

Our paper produces two key findings. First, in southern Bangladesh, targeting poor households using machine learning and mobile phone data (AUC = 0.61) is more accurate than community-based targeting (AUC = 0.58), but less accurate than proxy-means testing via household surveys (AUC = 0.82). Second, we provide the first direct comparison of targeting approaches based on cost-effectiveness, building on the welfare framework introduced by Hanna and Olken (2018). We show that it would be more cost-effective to use traditional approaches like proxy-means testing to target social protection programs with large budgets relative to the number of households screened, but algorithmic targeting approaches are worth considering for programs with

thinly stretched budgets (below $10-50 per household screened). Data from real-world government-run social protection programs suggest that most program budgets are sufficiently large that proxy-means testing is the most efficient targeting approach. However, 10-30% of countries in the World Bank ASPIRE database have small enough social protection budgets relative to the size of their population such that phone-based targeting would be preferred.

Robustness and Limitations The insights we present may be sensitive to the details of real-world social protection programs. First, our empirical analysis draws heavily on two specific programs implemented in Bangladesh and Togo, with household surveys and administrative cost data collected there. We have added context by using data from other programs (e.g., the World Bank's ASPIRE database), and through simulations of results under counterfactual parameters (e.g., to show how conclusions would differ if phone-based or community-based targeting were substantially more accurate, as in Figure S12). However, to make specific recommendations in individual countries, or to generalize across a wider range of environments, more work is needed to better calibrate the costs and accuracy of targeting approaches, especially novel digital approaches like phone-based targeting.

Second, our analysis is focused on a one-time program in which beneficiaries received transfers soon after the data used to determine eligibility was collected. In practice, PMT and CBT targeting sweeps are typically conducted only every few years (Barca and Hebbar, 2020). This lowers per-year screening costs, but targeting accuracy also typically deteriorates as data become out-of-date (Hillebrecht et al., 2023; Brown et al., 2018). For instance, Aiken et al. (2023b) estimate that PMT targeting accuracy decreases, on average, by 9 percentage points for each year that the PMT data are out of date. Aiken et al. (2022) show that the accuracy of phone-based targeting also degrades over time, as the underlying relationship between phone use and poverty changes.

Third, our measurement of costs focuses only on the financial costs of screening households. The private costs to households who participate in screening activities such as responding to household surveys or participating in

community meetings are not accounted for in our cost analysis. Imputing the value of people's time using survey data on hourly wages increases the cost of targeting via PMTs by approximately 9%. For CBTs, targeting costs would increase by 94% for households that participate in CBT meetings, although non-participating households would not incur such costs.[24] Accounting for these non-monetariy costs would make phone-based targeting look relatively more attractive.

A final limitation of our analysis is that we have abstracted away from the concern that households might strategically alter their behavior to game the targeting regime after the targeting mechanism has been implemented (Goodhart, 1975; Lucas, 1976). For instance, households might look for ways to manipulate the information that is collected in the PMT (Camacho and Conover, 2011; Banerjee et al., 2020), or change the way they use their mobile phones (Björkegren et al., 2020), in order to become eligible for benefits. Likewise, elite capture is a potential concern with CBTs (Han and Gao, 2019; Alatas et al., 2019); it is possible that community members might increasingly try to influence the decisions made by the selection committee with a CBT. Ex ante, it is not obvious which of the targeting processes would be most vulnerable to such strategic behavior. In principle, the "black box" nature of machine learning algorithms may make them more difficult for beneficiaries to game. At the same time, beneficiaries may prefer – or regulations may require – decision rules that are transparent (Walmsley, 2021).[25]

[24]We estimate respondents' opportunity costs of time by multiplying the average duration of PMT surveys and community meetings by the average hourly-income of an earning household-member in our sample. The latter is simply the average household-income of the households we surveyed divided by the average number of earners per household in 2022 reported by Bangladesh Bureau of Statistics (2023). Expressing this time cost as a proportion of the per-household variable cost of the PMT survey or community meeting provides estimates of how the corresponding targeting costs increase when accounting for respondents' time. The relatively large increase for CBT is partly because, although an enumerator's costs for a single CBT meeting are divided among many households, each meeting participant still spends as much time in the meeting as the enumerator does.

[25]For instance, the European Union's GDPR requires that beneficiaries receive "meaningful information about the logic involved, as well as the significance and the envisaged consequences of such processing for the data subject" (Selbst and Powles, 2017).

Beneficiary Satisfaction One final consideration that we do not account for in our cost analysis is the possibility that beneficiaries have preferences over targeting methods. Prior experimental comparisons of CBTs to PMTs have found mixed results: Alatas et al. (2012) find that community members randomly assigned to a CBT program in Indonesia were more satisfied with the targeting process than those assigned to a PMT; however, Premand and Schnitzer (2021) find that community members assigned to a PMT in Niger were more likely to perceive the process as legitimate than those assigned to a CBT. We collected data on people's satisfaction with the process and their perceptions of fairness for both community- and phone-based targeting, after transfers were delivered. We present those comparisons here, but there are several caveats to interpreting those results: (a) The value of PBT and CBT transfers were very different ($300 vs $9), (b) they were delivered at different times (9 months vs 2 months before the satisfaction survey), (c) we provided almost no information about the PBT, whereas a community meeting accompanied the CBT, and (d) unlike Alatas et al. (2012) and Premand and Schnitzer (2021), the targeting method was not randomly assigned; CBT transfers were made in a random subset of villages that had already received phone-based transfers.

We conducted the satisfaction survey in October 2024 with 1,100 households in villages that received both phone-based and CBT transfers. While most respondents remembered the programs, their understanding of how eligibility was determined was quite poor (see Appendix C and Appendix Table S5 for details). As shown in Appendix Table S6, satisfaction scores for CBT were significantly higher than for PBT: respondents were 27 percentage points more likely to report being "satisfied" or "very satisfied" (as opposed to "somewhat" or "not at all" satisfied) when asked about the CBT process than when asked about the phone-based process. They were also 35 percentage points more likely to perceive the CBT process as fair compared to their perception of the PBT process.[26] Those who actually received CBT transfers were especially

[26]Respondents were asked to give a yes or no response to the question, "In your opinion, was the selection process to receive cash aid in the program fair?"

fond of the CBT, and those who received phone-based transfers preferred the phone-based approach.

Concluding remarks Our analysis compares very different paradigms for poverty targeting, and shows how the "best" approach is likely to change as programs scale up. In particular, the cost-effectiveness of the program depends critically on both program budget and the size of the population being screened. Our results suggest that although phone-based targeting is less accurate than proxy-means testing, it can be a more efficient way to allocate social protection programs with low budgets relative to their scale. We hope that this and subsequent analysis can further elucidate if and when algorithmic techniques would be a useful addition to the toolbox of targeting approaches available to policymakers.

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Appendix - For Online Publication Only

A Construction of targeting approaches

A.1 Phone-based targeting

The phone-based targeting approach is implemented in a similar manner to past work on predicting poverty from mobile phone metadata (Blumenstock et al., 2015; Blumenstock, 2018; Aiken et al., 2022, 2023c). Pseudonymized mobile phone metadata (call detail records, or CDR) were shared with our research team by all four mobile network operators active in Cox's Bazar, for all phone numbers collected in our census of 100,000 households conducted in February 2023 (the census collected all phone numbers from all adult household members). These data included the following information, for five months from March 1 to July 31, 2023:

- Records of incoming and outgoing calls, including a pseudonymized identifier for the caller and recipient, date, time, and duration of the call, and GPS coordinates of the cell tower through which the call was placed and received
- Records of outgoing SMS messages, including a pseudonymized identifier for the sender and recipient, date and time of the message, and GPS coordinate of the cell tower through which the message was sent
- Records of mobile data usage, which we aggregate into the amount of mobile data (in megabytes) used by each subscriber per day

From these data sources, we calculate 1,578 "features" describing mobile phone use for each pseudonymized phone number in the dataset. We use open source python library cider[27] to calculate these features, which include information on call and text frequency, heterogeneity in contact networks,

[27] `https://global-policy-lab.github.io/cider-documentation/`

recharge patterns, mobility traces based on cell tower usage, and more (see cider's documentation for a complete list of features).

Next, we match the mobile phone features to the census and household survey, which are used to train our machine learning models and conduct the evaluation. For households that provided only a single phone number in the census (69% of households), each household is matched to the mobile phone metadata from the phone number provided. For households with multiple phone numbers recorded in the census (25% of households), the mobile phone metadata from the most senior member of the household is used.[28] The remaining 6% of households either did not provide a phone number of provided a phone number that did not produce any records in the March 1 - July 31 time period. These households were not included in the training of the ML model, and their phone-based poverty rankings were considered missing in the targeting evaluation, and therefore they are targeted last by the phone-based targeting approach. To build intuition, Table S2 shows the mobile phone features most correlated with measures of poverty from the survey: for example, the mean recharge amount is the feature most correlated with per capita consumption expenditures ($\rho = 0.19$), the PPI ($\rho = 0.23$), and the asset index ($\rho = 0.23$).

The dataset of mobile phone metadata features matched to poverty "labels" from the household survey (N = 4,820) is used to train the ML model. We train and evaluate the machine learning pipeline in the same way that we train and evaluate other ML-based targeting approaches, like the PMT (see Appendix A.3): We divide the matched features - household survey dataset (N

[28] An alternative approach to phone-based targeting for households with multiple phones would be to aggregate together poverty predictions from all phones to obtain a predicted measure of household poverty. Figure S13 shows the overall targeting accuracy of phone-based targeting when the ML model is trained on data from all phone numbers provided, and predictions are aggregated together for households with multiple phones (for single-phone households, the single prediction for that household's phone is still used). Three approaches to aggregation are tested: taking the average predicted poverty score, the minimum score, and the maximum score, as well as the status quo approach of taking the poverty prediction for the most senior household member. While there is little difference in the accuracy of these aggregation approaches — at least partly because relatively few (25%) households provided multiple phone numbers — taking the minimum score has lower targeting accuracy (31%) than the other three options (34%).

= 4,820) into a 75% training set and 25% test set. We train the model to predict per capita consumption expenditures on the training set using the gradient boosting model available in cider, with hyperparameters selected via three-fold cross validation. The main ML model is trained to predict log-transformed per capita consumption expenditure; we also test models to predict the PPI and asset index. We then produce predictions on the test set, which are used for evaluation. As with the other targeting methods, we repeat the process for 100 different random train-test splits, to produce confidence intervals in our downstream targeting evaluation. Sample weights are used in both training and evaluation.

In the phone-based targeting approach, households are targeted from poorest to richest based on their phone-based poverty prediction. Households without phones are targeted last.

A.2 Community-based targeting (CBT)

Community-based targeting (CBT) exercises were conducted in each of the 180 neighborhoods included in our study, following the protocol used by BRAC. The CBT protocol is summarized as follows. First, in neighborhoods of more than 100 households, enumerators split neighborhoods into contiguous segments of 50-100 households and conducted separate CBTs in each. Enumerators worked with senior community members to identify 12-25 households to join the meeting, inviting households from all walks of life and ensuring participation from women, students, farmers, businessmen, and laborers. Each meeting began with a "social mapping" exercise in which a community map was drawn with each household identified by name and occupation. Meeting attendees then worked together to rank the socio-economic status of all households in the community by placing index cards representing each household on a string in the order of their socio-economic status.[29] To make the CBT exercises consequential, participants were informed at the start of the meeting that the

[29] Our instructions to the meeting participants were based on the CBT used by BRAC, and noted, "... we will ask you to conduct a ranking of households in your community based on their socio-economic status."

20% poorest-ranked households would receive a one-time cash transfer of 1,000 Taka ($31.88 USD PPP) following the meeting.

The normalized ranking within each village is used to identify the poorest households for our community-based targeting method. The implicit assumption of this approach is that poverty distributions across villages are comparable. Households that are not ranked in the community-based targeting approach (0.4% of households) are considered to be targeted last for benefits by the CBT approach.

A.3 Proxy means test (PMT)

The PMT implementation follows standard approaches in the literature (Hanna and Olken, 2018; Brown et al., 2018). We use the following demographic and housing-related variables as PMT predictors:

- **Household head demographic variables:** Age, gender, marital status, highest level of education, worked in past seven days, disability status
- **General household demographic variables:** Household size, number of children under 10, number of children under 18, highest education level of any household member, union of residence
- **Housing variables:** Number of rooms, has a kitchen, has a stove, has electricity, has a toilet, ownership status of house, ownership status of land, main material of roof, main material of walls
- **Asset ownership variables:** TV, fridge, fan, stove, furniture, cell phone, solar panel, bicycle, rickshaw, vehicles, crop inventory, poultry, goats, cows, unpowered agricultural equipment, powered agricultural equipment, fishing nets, non-engine-powered boat, engine-powered boat, business assets, owned place of business, owned dwelling, owned residential land, owned agricultural land, cash on hand

Continuous variables are scaled to a 0-1 range and winsorized with a 99% limit. Categorical variables are converted into a set of mutually exclusive

dummy variables; we combine any categories that make up less than 1% of observations into a generalized "other" category for each variable.

We then fit a model to predict log-transformed per capita consumption from these input variables on the training set, and produce predictions on the test set (separately for each train-test split). We experiment with four options for the machine learning model underlying the PMT:

- **Simple linear regression:** Implemented with Python's statsmodels API via weighted least squares. We fit the regression model on the training set, and produce predictions for the test set.
- **Linear regression with step-wise forward selection of predictor variables:** For this option, the training set is again divided into a 50% true training set and a 50% validation set. We implement stepwise forward selection on the training set – that is, we search across all predictor variables to find the single best predictor of consumption (based on R^2 score on the test set), we then search across all remaining predictors to add a second for a two-predictor model, and continue adding predictors until the test-set accuracy decreases with additional predictors. Once this stopping criterion is met and the predictor subset is identified, we use Python's statsmodels API (via weighted least squares) to fit a final simple linear regression using only this subset of predictors on the entire training set, and produce predictions for the test set.
- **LASSO regression:** LASSO regression uses a regularization term to automatically perform feature selection to avoid overfitting to the training set. We implement the LASSO with scikit-learn's Lasso model, and tune the regularization parameter using three fold cross validation on the training set.
- **Random forest:** We use scikit-learn's RandomForestRegressor model, and tune hyperparameters via three fold cross validation on the training set. The ensemble size is chosen from [50, 100] and the maximum tree depth is chosen from [2, 4, 8].

Overall, we generally observe similar predictive performance of these different PMT variants: the LASSO is best with average $R^2 = 0.38$ (standard deviation 0.02), followed by OLS also at $R^2 = 0.38$ (standard deviation 0.03), then stepwise forward selection at $R^2 = 0.37$ (standard deviation 0.03), and finally the random forest at $R^2 = 0.33$ (standard deviation 0.02). In our main results we therefore show only the LASSO results, but in our supplementary results we show all four PMT variants. In general, these R^2 values are on the low end in comparison to reported R^2 values for PMTs elsewhere: for example, Brown et al. (2018) report R^2 values ranging from 0.32 in Ethiopia to 0.64 in Burkina Fasso and Hanna and Olken (2018) report R^2 values of 0.53 in Indonesia and 0.66 in Peru. One explanation for the low PMT R^2 in our context is the subnational and highly geographically concentrated nature of our survey — these other PMTs were trained and evaluated at a nationwide scale.

Figure S14 Panel A shows the PMT (using a LASSO regression) distribution for one example train-test split.

A.4 Geographic targeting

Bangladesh's most recent official poverty map is only available at the upazila (sub-district) level. (BBS, 2020) With only three upazilas in our household survey, geographic targeting at the upazila level is not a relevant targeting approach in our setting. We therefore use high-resolution poverty maps based on nontraditional data sources to simulate geographic targeting.

Our satellite-based poverty estimates come from the gridded Global Deprivation Index (GDI) released by NASA/Columbia's SEDAC center last year (CIESIN, 2021). The GDI uses subnational administrative datasets and gridded earth observation datasets to produce an "index of relative deprivation" in approximately a 1km global grid. The index consists of six components: (1) child dependency ratio from gridded population of the world datasets, (2) infant mortality rates from the global subnational infant mortality rates dataset, (3) the subnational human development index from the Global Data Lab, (4)

the ratio of built-up to non-built-up area using data from Facebook's High Resolution Settlement Layer and OpenStreetMap, (5) nighttime lights intensity from VIIRS, and (6) changes in nighttime light intensity from 2012 to 2020. The average of these six components makes up the GDI.[30]

We aggregate the GDI to three different geographic levels, for three variants of geographic targeting. For each level of aggregation, we talk the weighted average of GDI tiles contained (or partially contained) within the boundary, with weights determined by the population contained within the tile. The population density layer is also based on remote sensing and released by Meta (Tiecke et al., 2017). The three levels of aggregation are as follows, ordered from lowest to highest resolution:

- **Unions:** We use publicly available union shapefiles[31] to aggregate the GDI to the union (admin-4) level. These shapefiles do not contain urban wards, the admin-4 unit in urban areas. To obtain extents for the eight wards in our census dataset, we use the same process used to identify village and neighborhood extents, described in detail below. There are 23 admin-4 units in total for households in our household survey: 10 in Ramu, 5 in Ukhia, and 9 in Teknaf, ranging from 0.05-137 square km (median of 21 square km). 97% of admin-4 units overlap with at least one GDI tile, with the median containing 28 tiles. For the remaining 3% of admin-4 units, the poverty level assigned is that of the closest GDI tile.

- **Villages:** To our knowledge, there are no publicly available village shapefiles for Bangladesh. To calculate the boundary of each village, we take the convex hull of all GPS coordinates recorded for households in that village in the census. Any household that is not closer than 2km to at least 20 other households in the same village is considered an outlier, and not included in the process of calculating the convex hull. We then take the

[30] We prefer the GDI to the Relative Wealth Index (RWI) released by Meta (Chi et al., 2022) that has been used in previous work on remote sensing-based geographic targeting (Aiken et al., 2022; Smythe and Blumenstock, 2022) because RWI data are missing for much of the eastern portion of Cox's Bazar.

[31] `https://data.humdata.org/dataset/cod-ab-bgd`

weighted average of all GDI tiles overlapping the convex hull of the village. There are 105 villages in total in our household survey: 37 in Ramu, 25 in Ukhia, and 43 in Teknaf, ranging from 0.01-27 square km (median of 0.70 square km). 96% of villages contain at least one GDI tile, with the median containing four tiles. For the remaining 4% of villages, the poverty level assigned is that of the closest GDI tile.

- **Neighborhoods:** We repeat the same process to identify the convex hull of each neighborhood based on GPS coordinates recorded in our census. Again, any household that is not closer than 2km to at least 20 other households in the same neighborhood is considered an outlier, and not included in the process. There are 180 neighborhoods in total in our household survey: 60 in Ramu, 60 in Teknaf, and 60 in Ukhia, ranging from less than 0.01 square km to 3 square km. 94% of neighborhoods overlap with at least one GDI tile, with the median containing two tiles. For the remaining 6% of neighborhoods, the poverty level assigned is that of the closest GDI tile.

Figure S15 shows the poverty maps produced through this technique, at the union, village, and neighborhood level.

A.5 Poverty probability index (PPI)

We implement the Bangladesh PPI released by Innovations for Poverty Action, which was calibrated using the 2016-17 Household Income and Expenditures Survey (which is nationally representative). The PPI consists of a scorecard of ten questions: district (Cox's Bazar for all our households), housing members, children under ten, the highest grade completed by anyone in the household, ownership of a bicycle, refrigerator, and fan, construction material of household walls, electricity connection, and type of toilet used. In our data, all questions except for electricity and the number of children under 10 were collected in the census (the remaining two were collected in the household survey). The final score represents the probability that the consumption of the household

in question falls below the national poverty line. The mean PPI among our surveyed households is 54.18, with a standard deviation of 12.97. Figure S14 Panel B shows the distribution of the PPI in our household survey.[32]

A.6 Asset index

The asset index is constructed following Filmer and Pritchett (2001). We use principal components analysis (PCA, implemented with Python's wpca package) to obtain a vector representing the direction of maximum variation in asset ownership among each of the 26 assets collected in the survey (where each asset variable is a binary indicator for ownership of the asset). The PCA is fit using only the training set; we then project the data for each test set household onto this vector. Across 100 train-test splits, the first principal component explains on average 18.14% of the total variation in asset ownership (standard deviation of 0.22%). Figure S14 Panel C shows an example asset index distribution from one of the train-test splits.

A.7 Peer rankings

In our household survey, we included a peer rankings module. In this module, each household was asked to rank eight randomly selected households from their neighborhood, as well as themselves. Randomization was done to ensure that every household ranked eight other households, and each household would be ranked eight times. For each household j ranked by i, we asked i how well they knew household j (on a scale of 1-4),[33] and we asked i to rank the absolute welfare of j on a five-category scale.[34] Finally, we asked i to provide a relative

[32]The PPI is similar to other categorical or scorecard-based targeting approaches. A paticularly relevant one in the Bangaldesh setting is IFPRI's categorical targeting approach (Ahmed and Bakhtiar, 2023); however we do not include this approach in our analysis because it was designed for urban areas only.

[33]The options were, "1. Among my closest relatives; 2. Know very well; 3. Know a little bit; 4. Do not know"

[34]The prompt was, "Is the household headed by [NAME OF HOUSEHOLD HEAD] a family that has the most, a family that has a lot, a family that has neither a lot nor a little, a family that has little, or a family that has the least?"

ranking, from worst-off to best-off, of the eight households in i's list.

In the peer ranking module, if a household reported not knowing one of the households it was supposed to rank, they were not required to rank that household. As such, most households are not ranked exactly eight times — the median household is ranked four times by neighbors (plus once by themselves). 97% of households have at least one neighbor ranking, and 93% of households have at least one high-confidence neighbor ranking. Figure S16 shows the distribution of the number of times each household was ranked.

To determine the final peer ranking of each household j, we aggregate rankings by taking the average ranking of all households i that ranked j. We test six different approaches: one for each of the different types of ranking (absolute vs. relative), and one for each of three different variants based on the strength of the i-j connection: one that uses all rankings; one that only uses neighbor rankings (i.e., by dropping self-ranking); and one that uses self-rankings and rankings of neighbors only if i reported knowing j "very well" or better. We also test an absolute poverty ranking that uses only the self-ranking. Figure S17 compares the accuracy of each of these approaches to using the peer rankings data to target cash transfers.

Absolute welfare estimates. To obtain the community-based absolute welfare rating for each household, we simply take the average of the welfare ratings of all other households that rated it. Again, we produce three variants of this estimate: One for all ratings (including self-ratings), one for only neighbor ratings, and one for only high-confidence neighbor ratings (plus the self rating). We also look at using the self rating alone.

Relative welfare estimates. To obtain the community-based relative welfare ranking for each household, we use the HodgeRank algorithm, originally introduced by Jiang et al. (2011), and recently used for community-based targeting analysis by Bloch and Olckers (2021). Hodgerank aggregates pairwise comparisons between items (in our case, households), where each pairwise comparison represents an assessed "distance" between the two items (in our

case, the difference in wealth between the two households). To produce these assessed distances, for each ranked household, we take the distance between rankings for each pair of households, normalized by the total length of the ranking. Following Bloch and Olckers (2021), if any pairwise comparison appears more than once in our dataset (16% of pairwise comparisons), we use the average (normalized) difference in ranking as input to the algorithm.

The Hodgerank algorithm has the benefit of a "goodness of fit" measure describing the degree of local inconsistency in the underlying rankings relative to the aggregate ranking. In our analysis, local inconsistency ranges from 0.31 when all rankings are used, to 0.28 when only neighbor rankings are used, to 0.23 when only high-confidence neighbor rankings and self-rankings are used. The inconsistency values reported by Bloch and Olckers (2021) using data from Alatas et al. (2016) in Indonesia tend to be lower: the median inconsistency across neighborhoods is 0.15.

For both the welfare rankings we assume that any household without a ranking is considered richer than all ranked households for the purposes of targeting — that is, they would be missed in targeting based on community rankings. Figure S18 shows the distributions of the six targeting rankings.

A.8 Further Details on Data Privacy

In developing our phone-based targeting algorithm, we adopted a comprehensive set of security protocols to safeguard data confidentiality. First, we obtained informed consent from surveyed households to use their survey responses and phone usage data to determine their eligibility for a cash transfer program. Second, the analysis of raw Call Detail Records (CDRs) associated with their phone numbers was conducted exclusively by telecom operators on their own premises by personnel who already had authorized access to such data. Neither the research team nor any affiliates of the project — including personnel from GiveDirectly or the Government of Bangladesh (GoB) — were granted access to the raw CDRs. Instead, the research team provided the telecom operators with a list of relevant phone numbers alongside a set of

algorithmic instructions designed to extract 1,578 aggregated features from the corresponding CDRs. These features comprised summary statistics of household phone usage behaviors and thus, did not contain any data pertaining to individual calls or text messages. Finally, the telecom operators merged this feature dataset with a separate dataset containing encrypted variables derived from household surveys and fully anonymized the resulting dataset. The final anonymized dataset was securely stored on an isolated server on the premises of Aspire to Innovate (A2i, an entity of the Government of Bangladesh), where all subsequent data analyses were conducted. We submitted the data sharing protocol as part of our IRB application to the University of California Berkeley and received approval under CPHS Protocol 2023-02-16103. We also obtained explicit permission from Bangladesh Telecom Regulatory Commission to use the aforementioned phone data for our project and an additional IRB approval from the University of Dhaka.

This data sharing and handling protocol effectively minimized the potential for exposure of Personally Identifiable Information (PII) data. On one hand, it ensured that our project did not expose phone data with PII to the research team or GiveDirectly. On the other hand, the telecom operators did not have access to un-encrypted survey data. The government did not have access to either phone or survey data with PII, as they received access only to the anonymized dataset. Thus, given the robust anonymization and secure data handling practices, individual households could not be identified by the research team, GiveDirectly, telecom operators, or any government entity once the full set of survey and phone data were available together.

Once completed, the targeting algorithm identified beneficiary households from the full set of households listed in the census. A2i transmitted the list of hashed phone numbers of these selected households to the telecom operators, without disclosing any additional information. The operators then performed the de-hashing process to retrieve the original phone numbers, which were subsequently provided to GiveDirectly for the implementation of their cash transfer program.

B Simulating Counterfactual Targeting

B.1 Simulating Improved Community-Based Targeting

In our main analysis (Section 4), we find that phone-based targeting substantially out-performs community-based targeting (CBT) in Bangladesh. This raises the question: how accurate would the CBT need to be in order to out-perform phone-based targeting? To answer this question, we simulate an improved CBT by taking a weighted average of a household's CBT rank and its true consumption rank, weighting the consumption rank progressively higher to move CBT rankings closer to the correct rankings. Figure S12 Panel A reproduces Figure 7 including these simulations of the improved CBT, for four different accuracy levels. Once the CBT's accuracy substantially exceeds that of phone-based targeting (Spearman's $\rho = 0.50$, compared to 0.23 for phone-based targeting and 0.65 for PMT), the CBT is the best approach for budgets in the range of \$10-30 per household screened, using the median PMT variable cost from the literature (\$4.00).

B.2 Simulating Improved Phone-Based Targeting

Our main comparison between PMT and phone-based targeting is likewise impacted by the relative accuracy of the two methods. For example, in Togo — where phone-based targeting accuracy is higher ($\rho = 0.40$) than in Bangladesh ($\rho = 0.23$) — there is a broader scope of programs for which phone-based targeting achieves a higher utility impact than PMT (Figure 7). To more systematically show the relationship between the accuracy of phone-based targeting and the choice between phone-based targeting and PMT, we simulate improved phone-based targeting in the same way we simulate improved CBT: we take a weighted average of a household's phone-based targeting rank and its true consumption rank, weighting the consumption rank progressively higher to move phone-based targeting rankings closer to the correct rankings. Figure S12 Panels B (Bangladesh) and C (Togo) reproduce the results from Figure 7 including these simulations of phone-based targeting with higher accuracy. In

both Bangladesh and Togo, when the Spearman correlation between phone-based poverty estimates and consumption is around 0.20 (as in Bangladesh), programs with budgets under $15 per household screened should use phone-based targeting. As the correlation increases to 0.40 (as in Togo), phone-based targeting performs better for programs up to $40 per household screened. Table S4 further illustrates the impacts of improving the accuracy of phone-based targeting, showing the budget at which aid programs should switch from phone-based targeting to PMT targeting, as a function of the accuracy of the phone-based approach.

C Beneficiary Satisfaction

We conducted a short survey in October 2024 to assess how households perceived the two cash transfer programs that had occurred in their neighborhoods. The survey included 1,100 randomly selected households from the 180 neighborhoods in which the CBT program was conducted, which were a random subset of the 200 villages in which phone-based transfers were delivered by GiveDirectly. We used a stratified random sampling approach, through which we selected four households from each of the 180 neighborhoods, one household that had received only a CBT transfer, one that had received only a phone-based transfer, and two households that had received neither transfer. We prioritized households from the baseline survey, but if there were no such households that fit the stratification criteria in a particular neighborhood, we supplemented the sample with households from the broader census.

In the survey, each respondent was first asked a set of questions about the phone-based cash transfer program, and then subsequently asked the same set of questions about the CBT program. For each program, we asked respondents (i) whether they remembered the program, and (ii) if they were a beneficiary of the program. We then asked them (iii) to describe, in their own words, how they thought eligibility was determined. Finally, we asked two questions about (iv) their perceptions of the program:

1. How satisfied were you with how the approach determined who was eligible to receive cash aid? [Not at all satisfied, Somewhat satisfied, Satisfied, Very satisfied]

2. In your opinion, was the selection process to receive cash aid in the program fair? [Yes, No]

C.1 Recall and comprehension

Of the 1,100 survey respondents, 75% remembered the phone-based "program run by GiveDirectly where cash was delivered via mobile money in January-February", and 88% remembered the CBT "program run by our survey firm

where cash was delivered via mobile money and physical cash in June-July." 20% of respondents reported being a direct beneficiary of the phone-based program, and 64% reported knowing a beneficiary. A similar share of respondents reported being CBT beneficiaries (20%) or reported knowing a CBT beneficiary (69%).

While recall of the programs was high, comprehension was low, particularly for the phone-based program. Table S5 shows a sample of responses to the open-ended question they were asked about how they thought eligibility was determined for each of the two programs.

C.2 Satisfaction and perceptions of fairness

Respondent satisfaction and perceptions of fairness were substantially higher for the CBT process than for the phone-based targeting process. In particular, 58% of respondents report being "satisfied" or "very satisfied" with the CBT, while 32% were satisfied or very satisfied with the phone-based targeting process. (These estimates, as well as the others reported in this section, use sample weights to make responses representative of the beneficiary population). Likewise, 79% perceived the CBT to be fair, while 45% perceived phone-based targeting to be fair.

To better tease apart the factors that correlate with satisfaction and fairness, we regress the two measures of satisfaction on the targeting approach, with the specification:

$$\text{Satisfaction}_{i,m} = \beta \text{CBT}_{i,m} + \mu_i + \epsilon_{i,m} \tag{2}$$

In these regressions, $\text{Satisfaction}_{i,m}$ is a binary variable indicating the satisfaction (1 if satisfied or very satisfied; 0 otherwise) of respondent i for targeting method m (phone-based vs. CBT). $\text{CBT}_{i,m}$ is a binary variable that takes the value 1 for questions about the CBT and 0 for the corresponding question about phone-based targeting. We include a respondent fixed effect μ_i to isolate differences within a given respondent in satisfaction across the two different targeting methods.[35] The main coefficient of interest, β, tracks

[35]Results without respondent fixed effects are similar but less precise.

respondents' propensity to indicate greater satisfaction with the CBT relative to the same question about phone-based targeting.

In some specifications, we also include interaction terms between household characteristics X_i and the CBT dummy. These interaction terms help us understand whether certain types of respondents are systematically more likely to prefer the CBT to phone-based targeting:

$$\text{Satisfaction}_{i,m} = \beta \text{CBT}_{i,m} + \gamma(\text{CBT}_{i,m} * X_i) + \mu_i + \epsilon_{i,m} \tag{3}$$

Results in Table S6 indicate a general preference for CBT. In the first specification (columns 1 and 3), corresponding to equation (2), we observe that on average, households are 26.5 percentage points more likely to report being satisfied with the CBT process than with the phone-based process, and are 35.2 percentage points more likely to consider the CBT process fair, relative to the phone-based process. In both cases, where the specifications include respondent fixed effects but no other control variables, $p < 0.01$.

In columns 2 and 4, corresponding to equation (3), we see that the relative evaluation of CBT vs PBT varies greatly depending on respondent characteristics, such as whether this household actually received a PBT or a CBT transfer. Perhaps unsurprisingly, beneficiaries' perceptions of a targeting method and program tend to be more positive if they received benefits from it (rows 2 and 3). We also observe that people who participated in the CBT process (row 4), and who are aware of the CBT program (row 5), are more satisfied with the CBT process, and more likely to perceive the CBT process as fair.

D Supplementary Figures and Tables

Figure S1: Project timeline

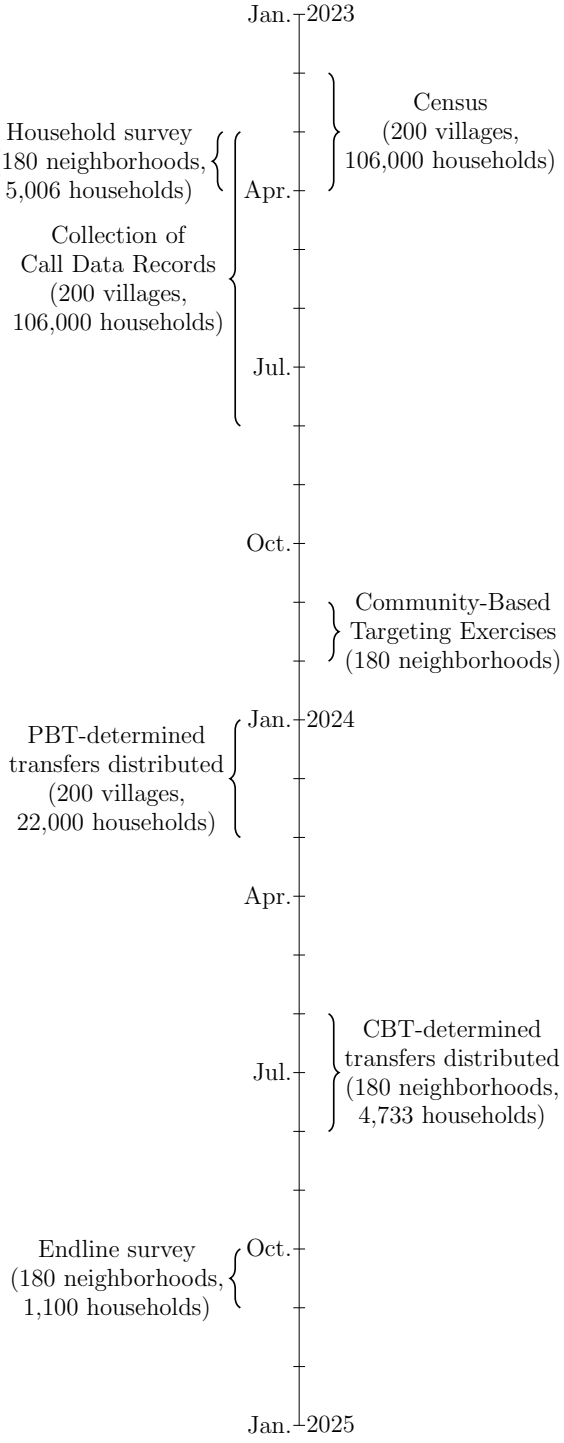
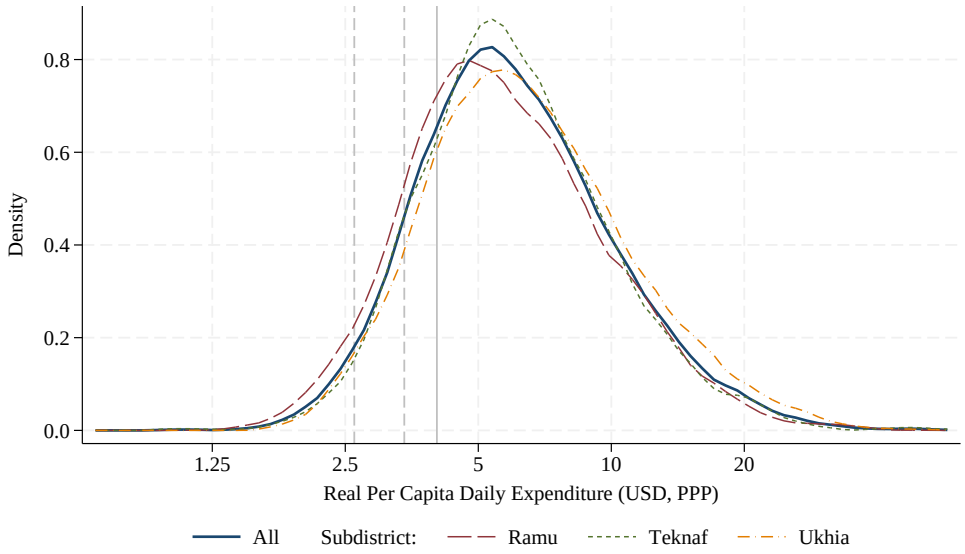
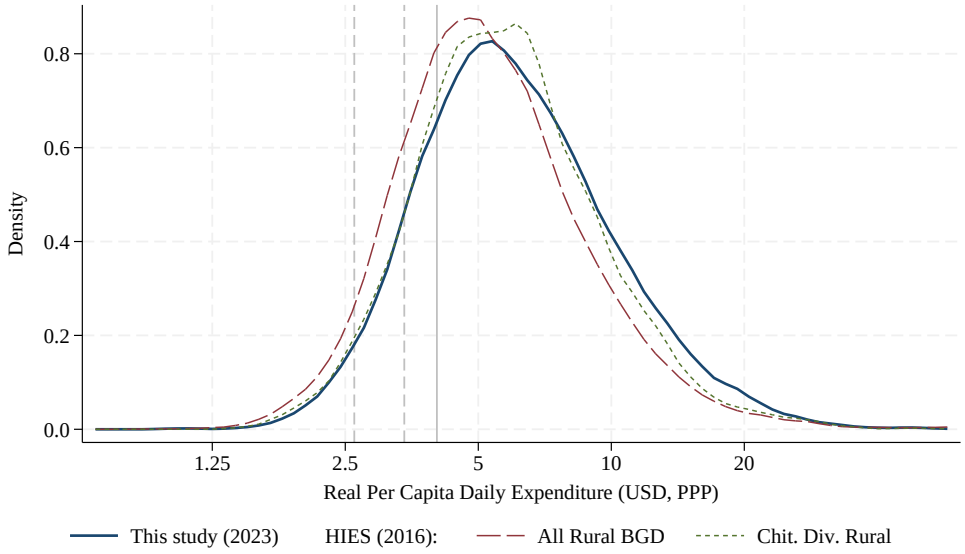


Figure S2: Density of household real per-capita daily consumption

(a) By subdistrict (upazila)

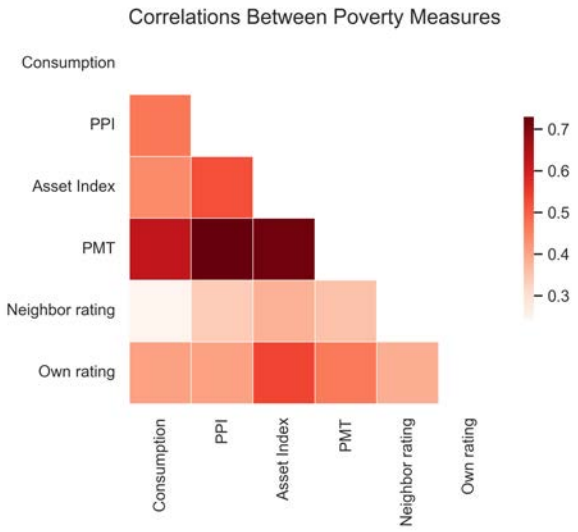


(b) Compared to 2016 HIES



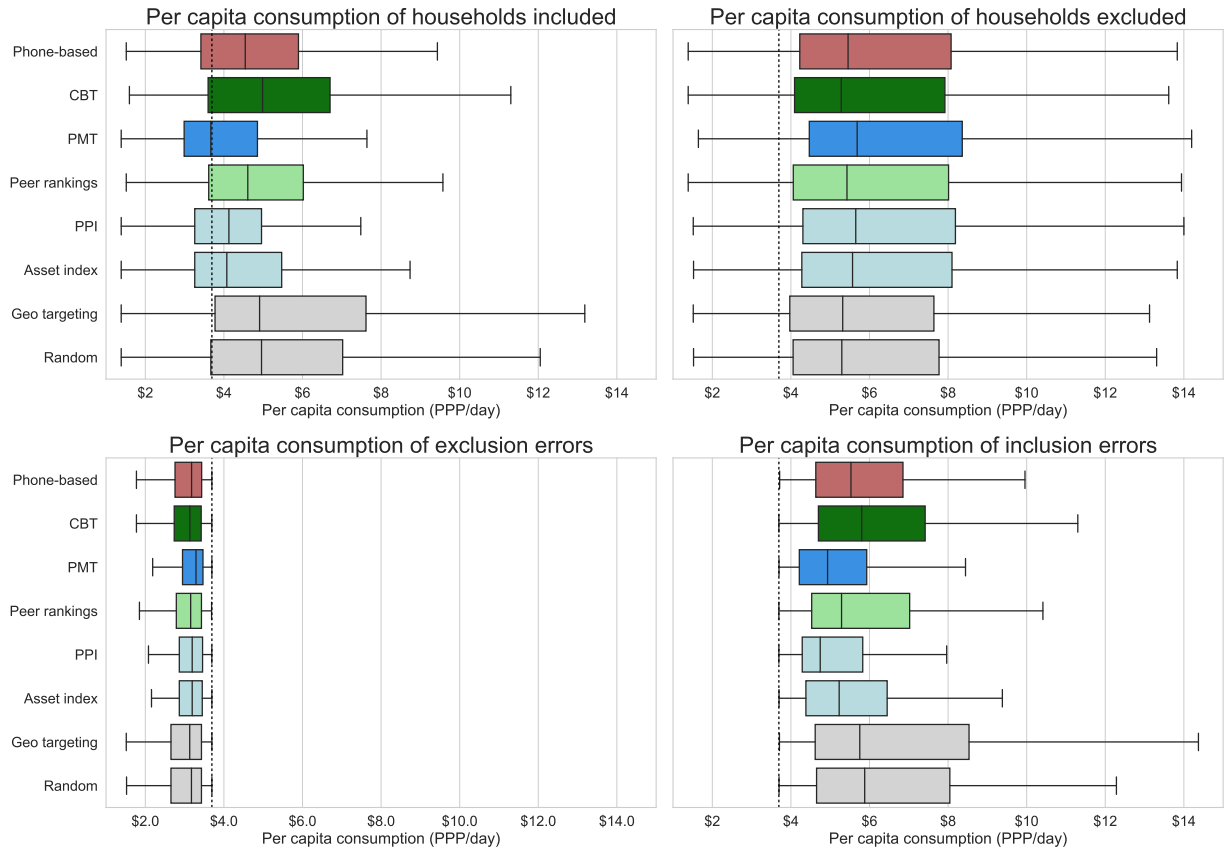
Notes: these figures plot the density of real household per-capita daily consumption in 2023 USD (PPP) from the household survey (solid line). The solid vertical line represents the 21st percentile (PPP USD 4.03). The dashed vertical lines indicate the lower and upper poverty lines for rural Bangladesh (PPP USD 2.62 and 3.40, respectively). In the top panel, we additionally plot the density by sub-district. In the bottom panel, we plot the same variable from the 2016 Bangladesh Household Income and Expenditure Survey (HIES) for two sub-groups, all rural households in Bangladesh (long dash) and rural households in Chittagong division (short dash). Our study was conducted in three sub-districts (upazilas) of Cox's Bazar district (zila) in Chittagong. Observations from the HIES are weighted using the 2016 HIES household inverse probability weights. 2016 nominal consumption in BDT is converted to 2023 BDT using the Bangladesh CPI, and then to USD at purchasing power parity at the mean 2023 PPP exchange rate for personal consumption of 30.7 BDT/USD.

Figure S3: Correlation between key poverty outcomes



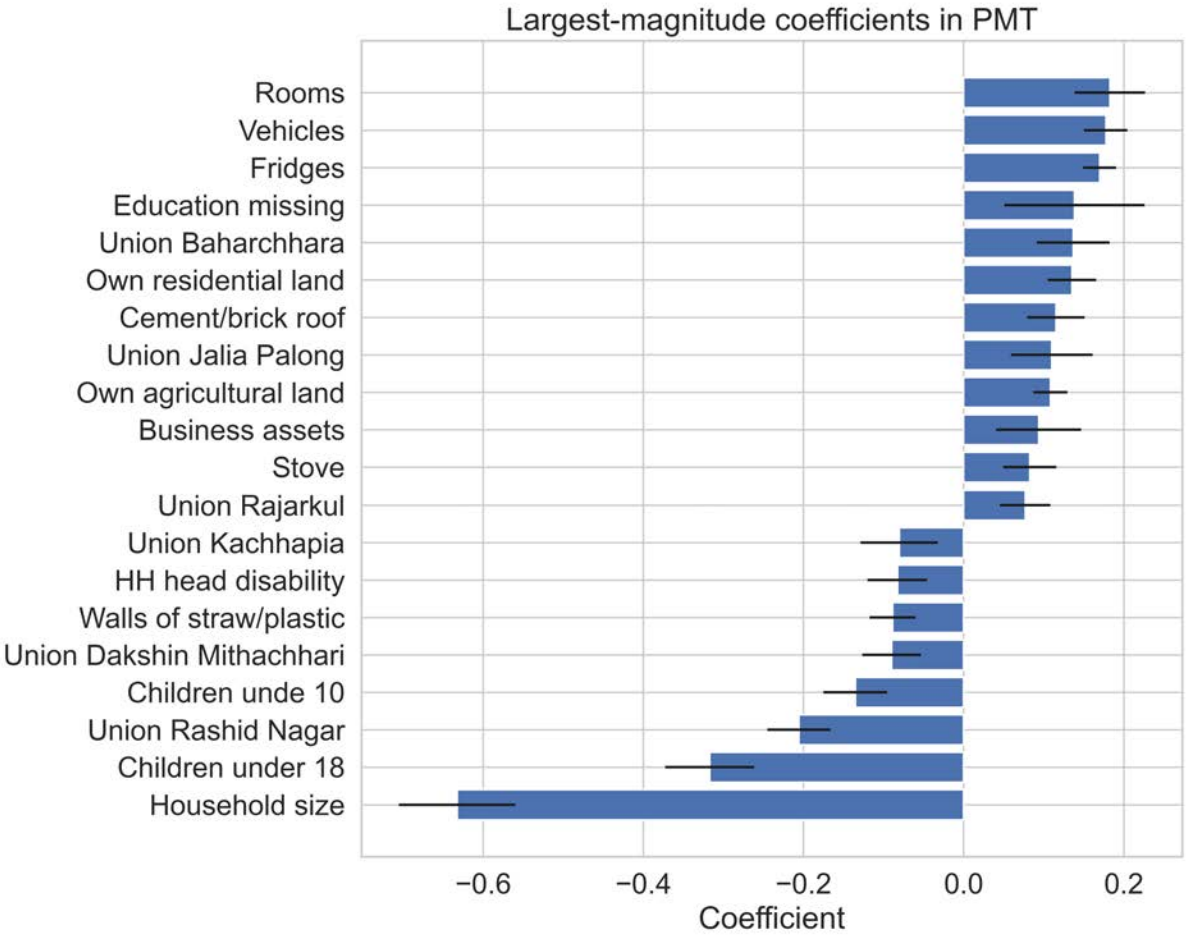
Notes: this figure displays a matrix of correlations between key poverty outcomes from the baseline household survey.

Figure S4: PCE distributions by inclusion / exclusion and targeting method



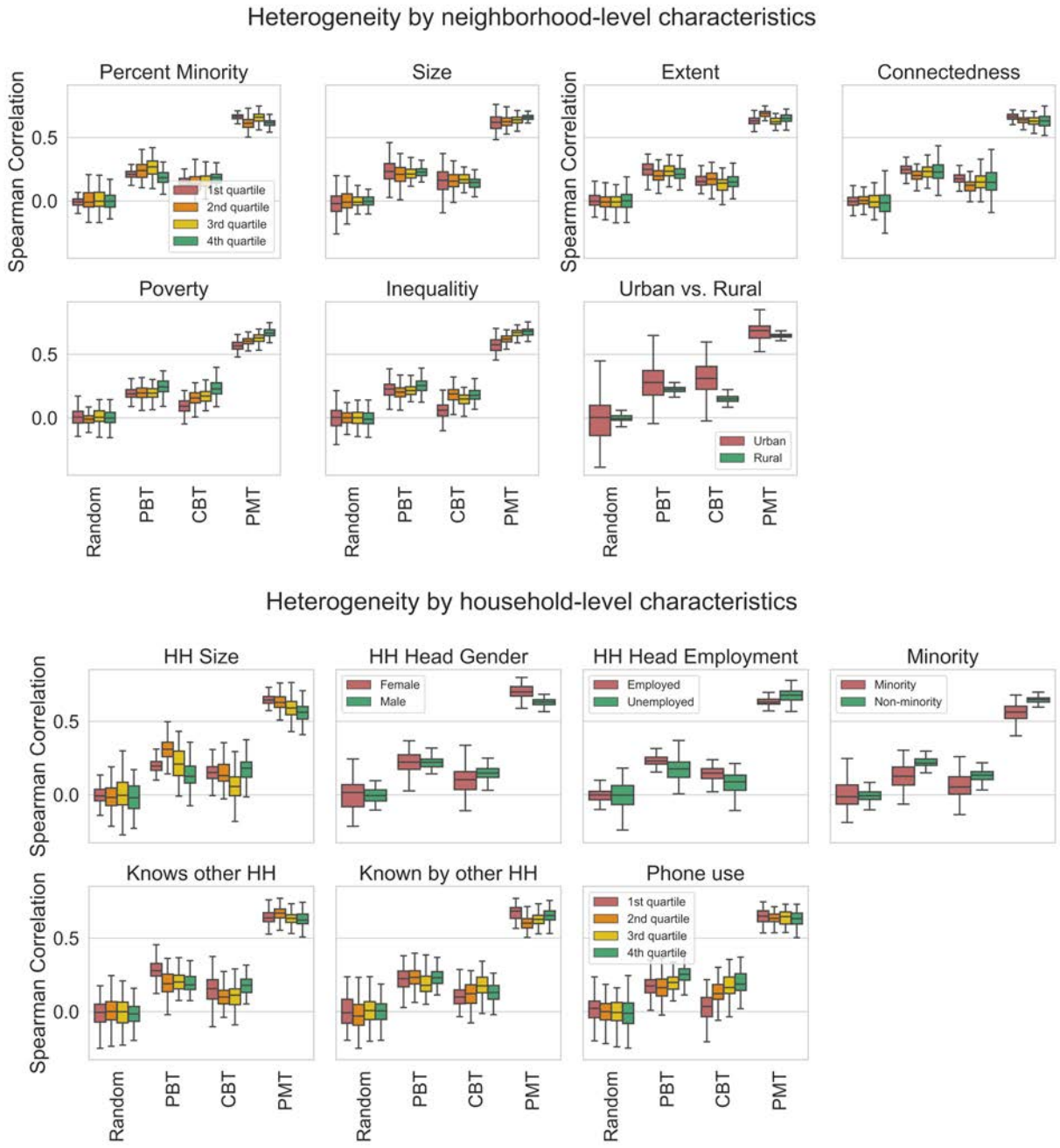
Notes: these figures depict the distribution of PCE per capita, by targeting method, for households included (top left), excluded (top right), wrongly excluded (bottom left), and wrongly included (bottom right). The boxes denote the 25th, 50th and 75th percentiles, while the whiskers capture the maximum and minimum. The vertical dashed line denotes the targeting cutoff (21st percentile). For comparison, the distribution under completely random targeting is shown as the last item in each panel. A better-performing targeting method will tend to: include poorer households, shifting the distribution in the top left panel to the left; exclude less-poor households, shifting the distribution in the top right to the right; incorrectly exclude households closer to the cutoff (from below) rather than the poorest households, so the distribution in the bottom left panel will be compressed against the cutoff line from the left; incorrectly include households closer to the cutoff (from above) rather than the least-poor households, so the distribution in the bottom right panel will be compressed against the cutoff line from the right.

Figure S5: PMT variables with largest estimated coefficients in LASSO



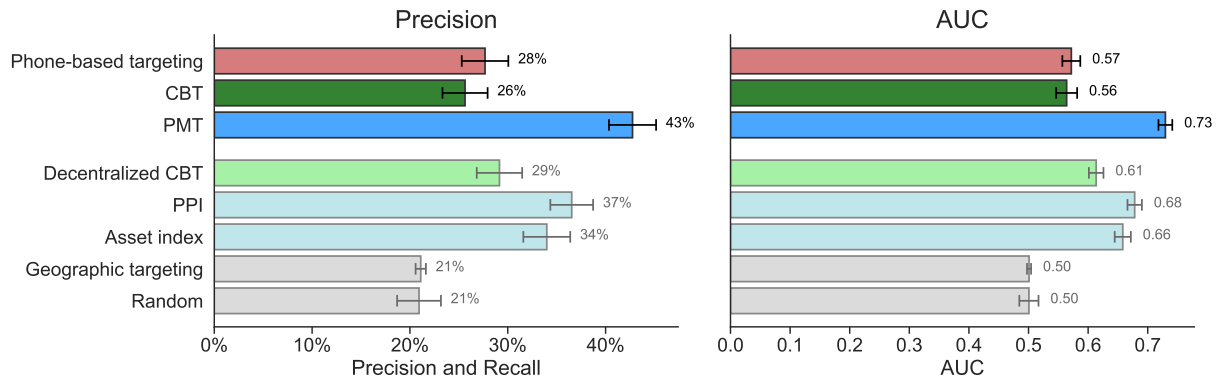
Notes: this figure displays the 20 PMT variables with the largest (in absolute value) coefficients in the LASSO estimation. Continuous variables are scaled to a 0-1 range. Coefficients are averaged over all 100 train-test splits, with error bars showing two standard errors above and below the mean coefficient across the 100 splits.

Figure S6: Heterogeneity in targeting accuracy



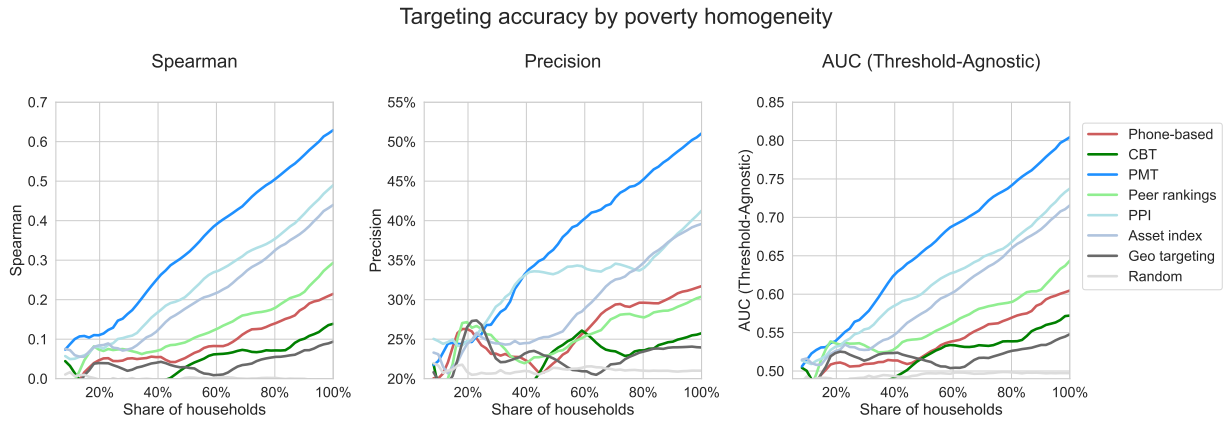
Notes: these figures plot heterogeneity in targeting accuracy by neighborhood-level characteristics (top row) and household-level characteristics (bottom row). Each plot shows the distribution of Spearman correlations (over the 100 random train-test splits) for each group.

Figure S7: Targeting accuracy comparison within neighborhood



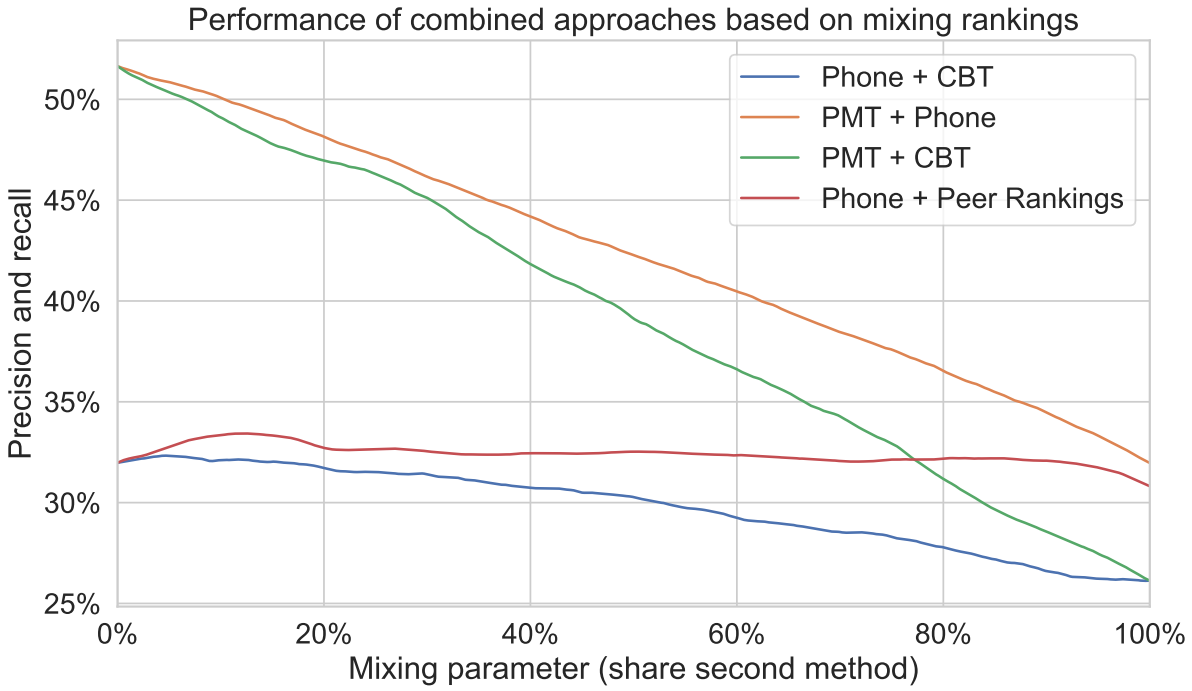
Notes: these figures compare accuracy by method for identifying the poorest households within neighborhood, rather than across the entire population, as in Figure 2. Accuracy based on precision and recall for identifying the 21% consumption-poorest households in each neighborhood (left), and area under the ROC curve (right). Accuracy is calculated over 100 random train-test splits, and error bars show two standard deviations above and below the mean for each metric.

Figure S8: Targeting accuracy by poverty homogeneity



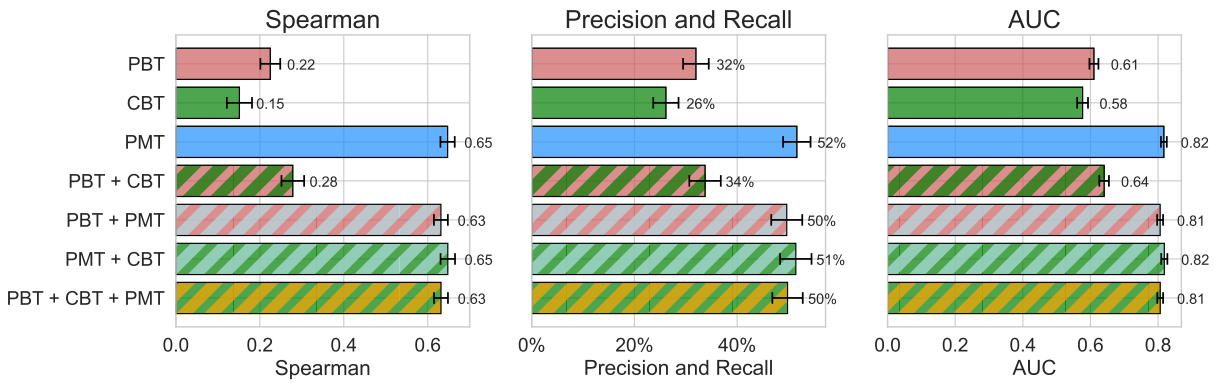
Notes: these figures compare the accuracy of each targeting method as a function of the poverty homogeneity of the population. The x-axis represents the share of households from our survey included, ranked by poverty: thus 20% indicates restricting the targeting evaluation to the 20% poorest households in our survey.

Figure S9: Accuracy of combining rankings across methods



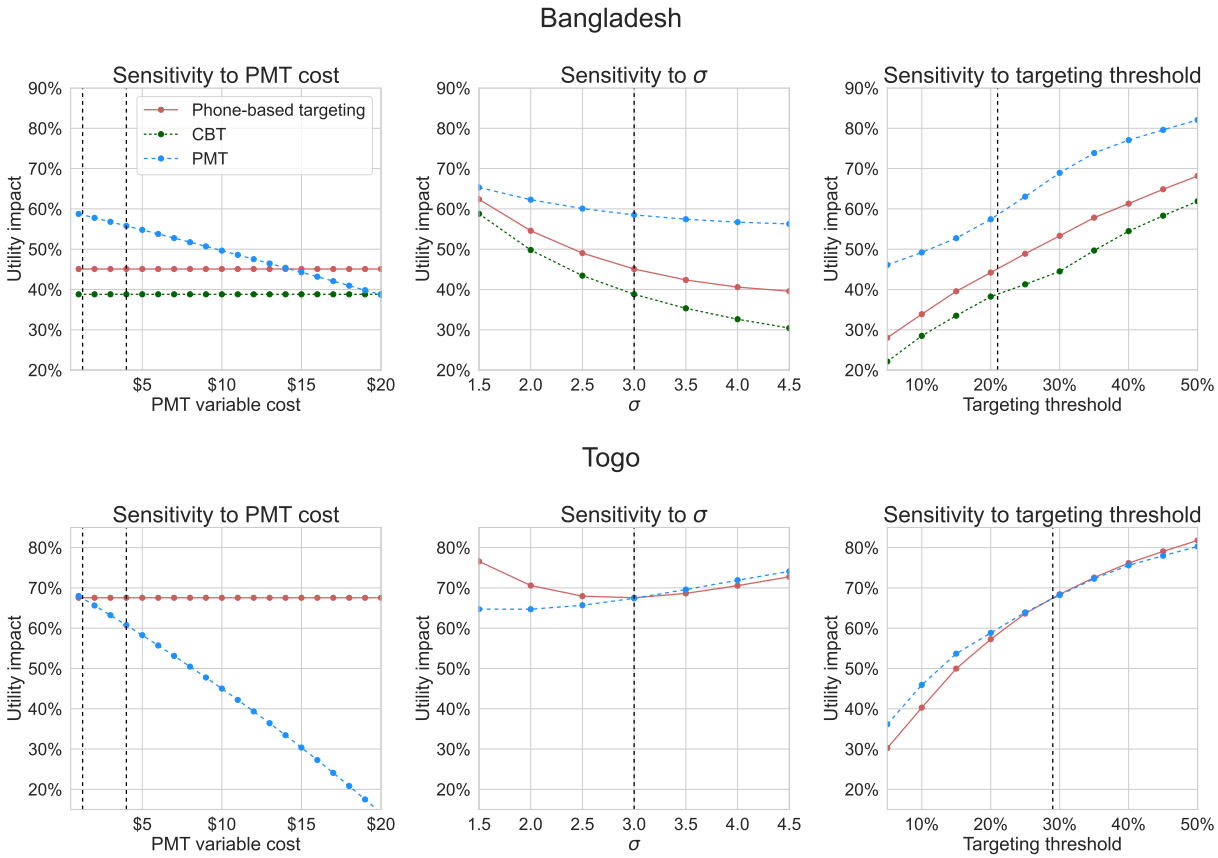
Notes: Figure shows the accuracy of targeting approaches that combine rankings from two different data sources, following the methods described in Section 3. The x-axis represents the "mixing parameter": the share of rankings that are taken from the second method in the pair (as opposed to the first). Four combined methods are tested: phone + CBT rankings (where the x-axis represents the share of rankings taken from the CBT), PMT + phone rankings (x-axis represents the share of rankings taken from phone-based targeting), PMT + CBT rankings (x-axis represents the share of rankings taken from the CBT), and Phone + Decentralized CBT (x-axis represents the share of rankings take from the peer ranking approach). Precision and recall measures are the average over 100 bootstrap simulations.

Figure S10: Accuracy of combining data across methods



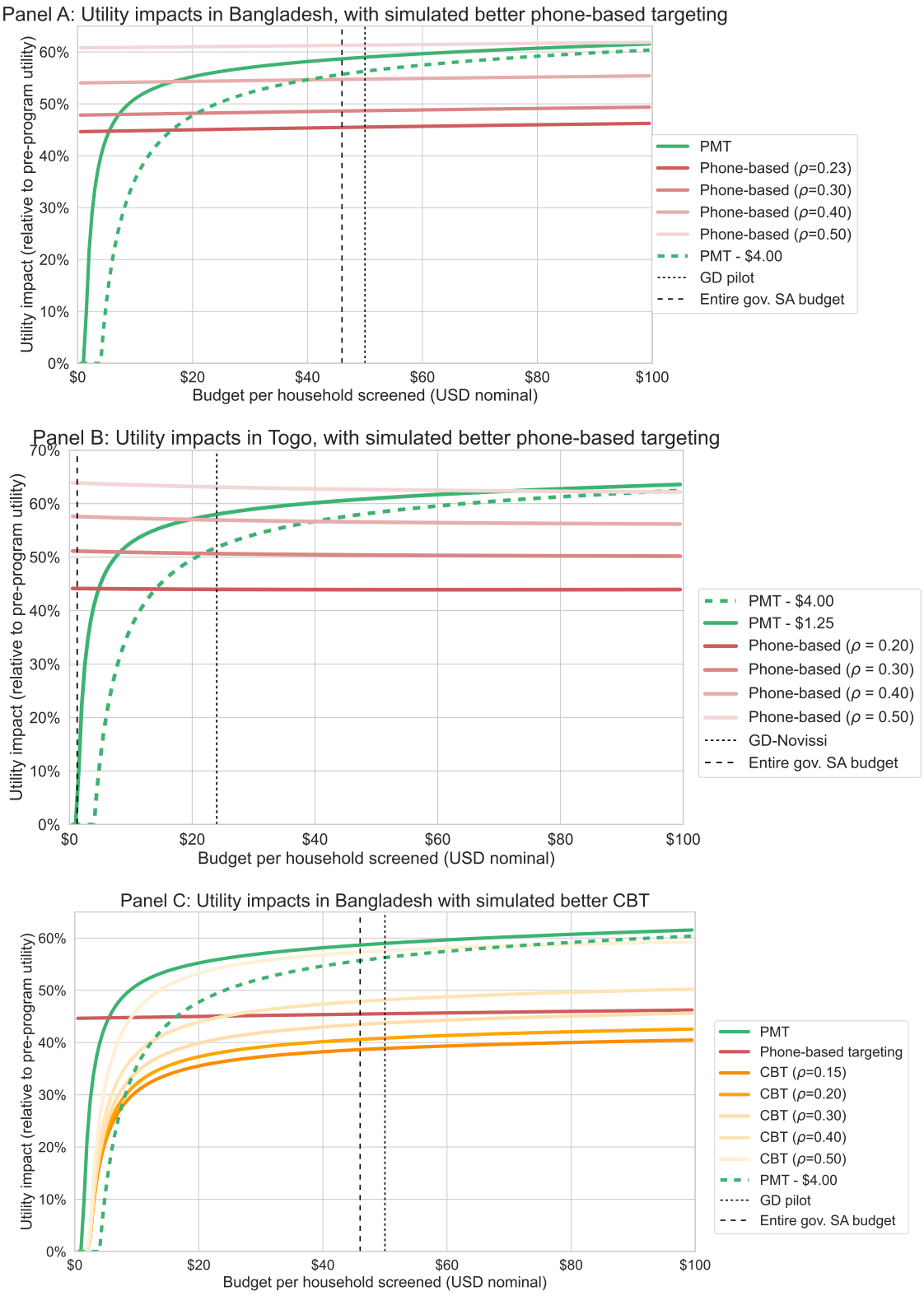
Notes: this figure depicts the accuracy of ML-based approaches combining data sources into a single targeting approach, following the methods described in 3.

Figure S11: Sensitivity of relative performance of targeting methods



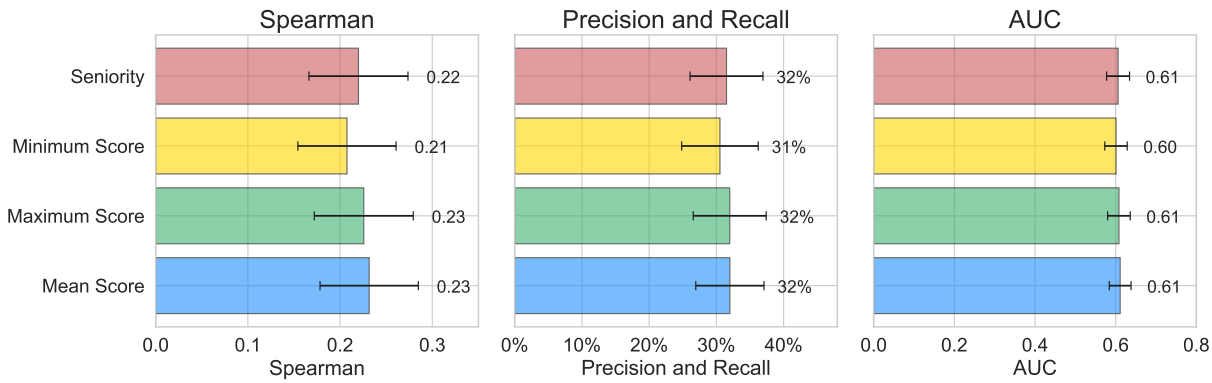
Notes: these figures show how the relative performance of PMT and CBT differs as we vary key parameters: the PMT variable cost (left in each row), σ (the coefficient of relative risk aversion for the CRRA utility function, center in each row), and the targeting threshold (share of households included, right in each row). Top: GiveDirectly program in Bangladesh (using the same data as the left panel of Figure 5). Bottom: GD-Novissi program in Togo (using the same data as the right panel of Figure 5). Dashed vertical lines indicate the values of these parameters used in the main analysis (e.g., Figures 5 and 7).

Figure S12: Sensitivity of relative performance to increases in accuracy



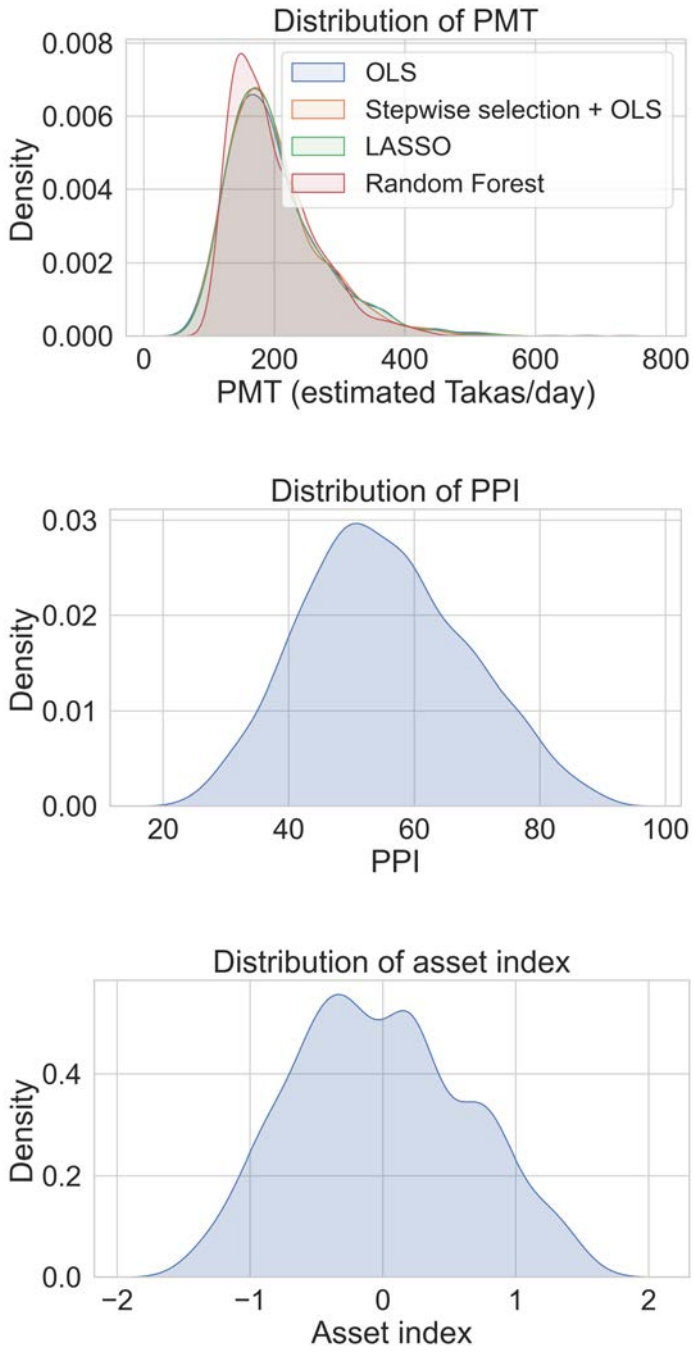
Notes: Figures replicate Figure 7 with the addition of simulated improvements in accuracy of phone-based targeting (top two panels) and community-based targeting (bottom panel). See Appendix B for details on how higher-accuracy CBT and phone-based targeting methods are simulated.

Figure S13: Households with Multiple Phones



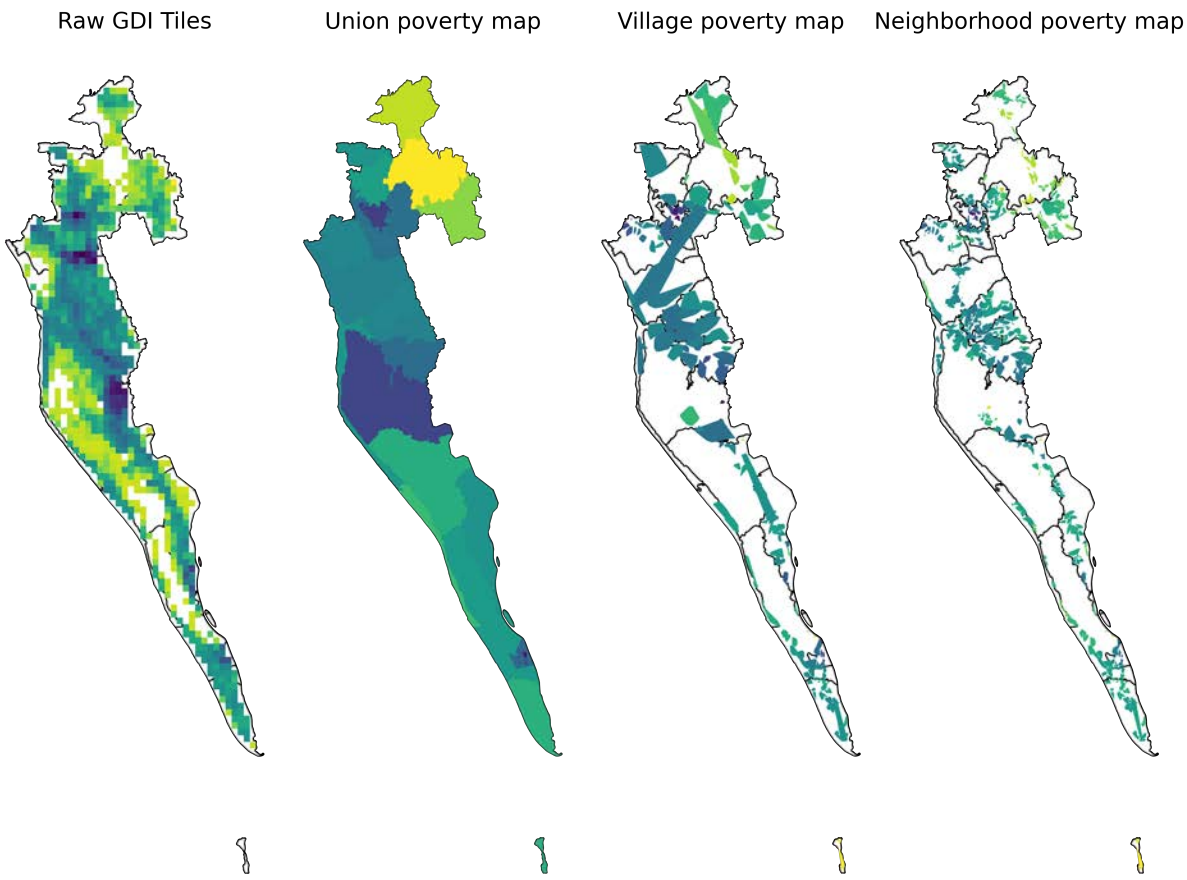
Notes: these figures present accuracy metrics for four different approaches to aggregating phone-based predictions for households with multiple phones. Targeting accuracy is calculated using the household survey dataset, as in the main targeting evaluation (Figure 2), and the approach for households providing only a single phone number (68%) or no phone numbers (3%) is unchanged. However, for households providing multiple phone numbers (29%), different approaches to aggregating poverty predictions from those phone numbers are tested: taking the prediction from the most senior member (as is implemented in the main targeting evaluations in this paper), taking the mean across predictions, taking the minimum across predictions, and taking the maximum across predictions.

Figure S14: Distribution of proxy measures



Notes: these figures present kernel density estimates showing the distribution of the PMT (left, with four versions corresponding to the four machine learning models tested), PPI (middle), and asset index (right), for one example train-test split.

Figure S15: Poverty maps

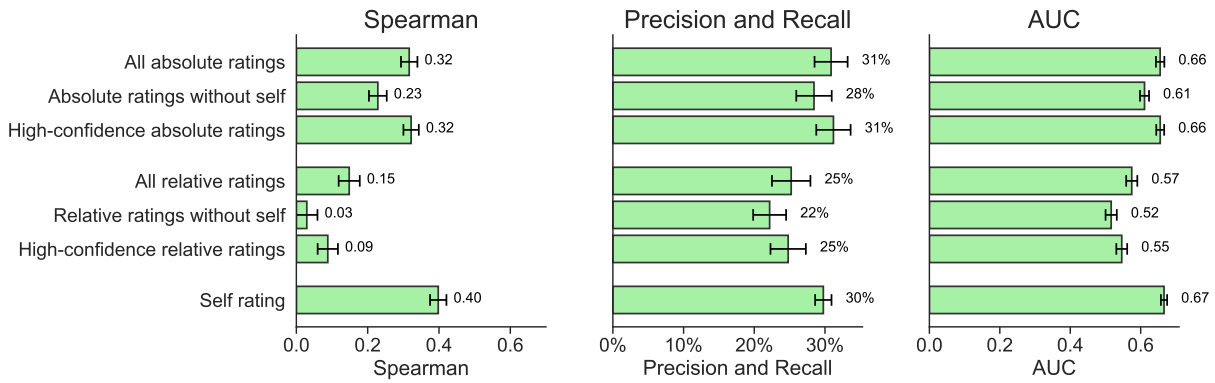


Notes: these figures display poverty maps produced by aggregating the Global Deprivation index (GDI) at the union, village, and neighborhood level, as described in Appendix A.

Figure S16: Distribution of rankings per household

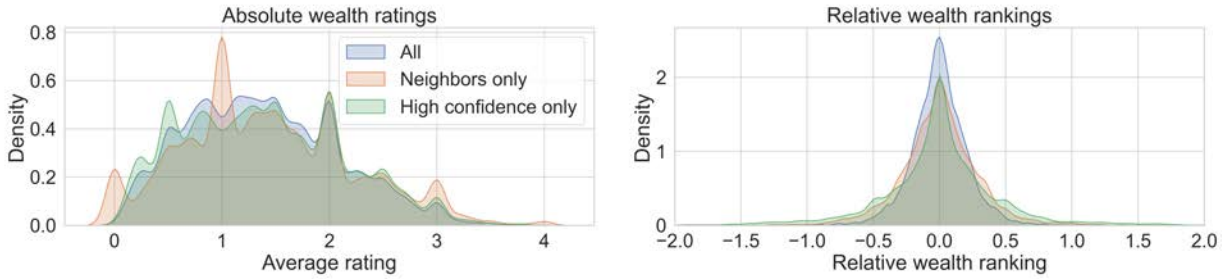
Notes: Figures display the number of rankings per household obtained in the peer rankings module of baseline survey, when keeping all rankings (left), only peer rankings (middle), and only high-confidence rankings (right).

Figure S17: Accuracy of peer ranking approaches



Notes: these figures show the accuracy of different approaches to aggregating peer ranking data for targeting. The first three sets of bars show the accuracy of approaches based on absolute ratings: each household is asked about the level of poverty of eight other households. The second three sets of bars show the accuracy of approaches based on relative rankings: each household is asked to order the eight other households in terms of poverty. The final bar shows the accuracy of self-assessments of poverty.

Figure S18: Distribution of aggregated peer rankings



Notes: these figures display distributions of aggregated peer rankings produced by averaging absolute ratings of wealth (left) and using the HodgeRank algorithm to aggregate relative rankings of wealth (right)

Table S1: Summary statistics from household survey

Variable	**Mean**
Panel A: Consumption	
Per Capita Daily Consumption (Takas)	215.27 (131.90)
Per Capita Daily Consumption (USD PPP)	6.48 (3.97)
Panel B: Additional survey-based poverty proxies	
PPI	54.75 (12.63)
Asset Index	0.00 (0.66)
PMT (Inferred Takas)	198.61 (70.54)
Panel C: Neighbor and self-assessments of poverty	
Neighbor-based poverty rating (1-5)	2.39 (0.79)
Self-assessed poverty rating (1-5)	2.22 (0.82)
Panel D: Household characteristics	
Household members	4.99 (1.97)
Number of rooms	2.67 (1.28)
Electricity access	0.82 (0.38)
Own house	1.18 (0.72)
Panel E: Household head characteristics	
Female	0.17 (0.38)
Age	41.85 (13.69)
Worked in past week	0.80 (0.40)
Has a disability	0.04 (0.20)

Notes: this table displays summary statistics from the baseline household survey. Standard deviations are shown in parentheses.

Table S2: Correlations between mobile phone features and poverty measures

	Per capita consumption		**Asset Index**	
	Feature	ρ	*Feature*	ρ
1	Mean recharge value	0.19	Mean recharge value	0.23
2	Max recharge value	0.16	Max recharge value	0.19
3	Min recharge value	0.14	# Call contacts (weekdays	0.18
4	# Days with mobile data use	0.13	# Call contacts (weekday, daytime)	0.18
5	# Call contacts (weekday, daytime)	0.10	# Days with mobile data use	0.17
6	# Call contacts (daytime)	0.10	# Call contacts (weekday)	0.17
7	# Call contacts (weekday)	0.10	# Call contacts	0.17
8	# of divisions visited	0.10	% of calls at night (weekday)	-0.17
9	# of subdistricts visited	0.10	% of calls at night	-0.17
10	# Call contacts (anytime)	0.10	# Weekend call contacts (daytime)	0.16
N		4,820		4,820

Notes: Mobile phone features with the strongest bivariate correlations with each poverty measure from the survey are shown, in descending order, calculated using the dataset of mobile phone features matched to household survey data ($N = 4{,}820$). A "recharge" occurs when someone adds credit (of monetary value) to the SIM card, which can be used to make calls. "Call contacts" refer to the number of unique phone numbers with which the phone made incoming and outgoing calls. "# of divisions/subdistricts" refer to the number of unique geographic jurisdictions visited by the SIM, based on observed cell tower connections. "Days with mobile data use" refers to the number of unique days that the SIM card owner is observed to use mobile data.

Table S3: Correlates of inclusion and exclusion for each targeting method

	Phone-based	**CBT**	**PMT**	**Peer rankings**
Panel A: Household characteristics				
HH head female	0.011 (0.042)	0.035 (0.042)	0.065 (0.038)	0.039 (0.041)
HH head age	0.001 (0.013)	0.004 (0.013)	-0.065 (0.012)***	-0.029 (0.013)*
HH head employed	0.023 (0.032)	0.010 (0.033)	0.038 (0.030)	0.004 (0.032)
HH head minority	-0.061 (0.072)	-0.003 (0.072)	0.003 (0.066)	-0.070 (0.071)
HH head widow/widower	-0.018 (0.057)	0.140 (0.057)*	0.005 (0.052)	0.173 (0.056)**
HH size	0.006 (0.012)	-0.025 (0.012)*	0.154 (0.011)***	-0.040 (0.012)**
Connectedness (in)	-0.036 (0.015)*	0.003 (0.015)	-0.008 (0.014)	-0.013 (0.015)
Connectedness (out)	-0.020 (0.009)*	-0.004 (0.009)	0.023 (0.008)**	0.016 (0.009)
Own phone	0.319 (0.062)***	-0.068 (0.062)	0.006 (0.057)	-0.135 (0.061)*
Phone transactions	-0.086 (0.012)***	0.002 (0.012)	-0.043 (0.011)***	0.004 (0.012)
Food consumption share	0.060 (0.012)***	0.029 (0.012)*	0.071 (0.011)***	0.049 (0.012)***
Panel B: Neighborhood characteristics				
# of Households	0.064 (0.015)***	-0.005 (0.015)	0.024 (0.013)	0.034 (0.015)*
Land area (square km)	-0.030 (0.014)*	0.012 (0.014)	-0.019 (0.013)	-0.008 (0.014)
Density	0.012 (0.016)	-0.004 (0.016)	-0.026 (0.015)	-0.035 (0.016)*
Urban	0.025 (0.098)	0.018 (0.098)	0.057 (0.090)	0.009 (0.097)
% Minority	-0.006 (0.025)	-0.003 (0.025)	-0.042 (0.023)	0.012 (0.025)
Connectedness	0.059 (0.019)**	0.005 (0.019)	0.024 (0.017)	-0.004 (0.019)
Average consumption	-0.013 (0.014)	0.002 (0.014)	-0.089 (0.013)***	0.005 (0.014)
Inequality (Gini)	0.007 (0.014)	-0.001 (0.014)	0.041 (0.013)**	-0.025 (0.014)
Constant	-0.102 (0.067)	0.253 (0.068)***	0.150 (0.062)*	0.324 (0.067)***
N	1,252	1,252	1,252	1,252

Notes: Results of regressions for which types of households are selected by each targeting method (using one train-test split). The dependent variable of each regression an indicator for whether a household was targeted by the method in question; the independent variables shown are a representative set of demographic and neighborhood characteristics. Regressions are run jointly with all explanatory variables in the first column. All explanatory variables are standardized. Connectedness (under neighborhood characteristics) represents the average self-reported knowledge that households have of other households in their community, elicited during the peer rankings exercise in our household survey. Connectedness (in) under household characteristics represents the average knowledge that other households had of the household in question during the peer ranking exercise; connectedness (out) represents the average knowledge that the household in question had of other households in their community. Regressions are run using data from a single train-test split. Standard errors are in parentheses.

Table S4: Policy implications of phone-based targeting accuracy

	Low-cost PMT ($1.25)		**High cost PMT ($4.00)**	
Spearman	**Bangladesh**	**Togo**	**Bangladesh**	**Togo**
0.20	$4	$4	$15	$13
0.30	$6	$7	$20	$21
0.40	$17	$19	$40	$39
0.50	$98	$71	Over $100	$94
0.60	Over $100	Over $100	Over $100	Over $100

Notes: Budgets per household screened at which aid programs should switch from phone-based targeting to PMT, as a function of the accuracy of phone-based targeting accuracy (PMT accuracy is held fixed). Calculations are made using the simulated improved phone-based targeting methods from Figure 7, separately for a PMT with variable costs of $1.25 per household screened (left) and $4.00 per household screened (right).

Table S5: Example responses to questions about comprehension of eligibility criteria

Respondent	**How they thought eligibility was determined in GD program**	**How they thought eligibility was determined for CBT-based transfers**
1	I have no idea about this program. So I don't know how the eligibility criteria were determined.	Most of the rich got aid, so don't think it's appropriate.
2	I don't know how they decided because no one told me about it.	I know that the real poor families have been determined through meetings or getting together in the area.
3	Don't know how eligibility is determined under this program.	I heard they had organized a meeting but I am not familiar with the process they used for checking the eligibility for cash assistance.
4	I don't know anything about this programme. I don't know how the eligibility for the aid programme was determined.	She doesn't know the selection process, but they heard it helped 20% of poor people.
5	I don't know about this method, so I'm not sure how the selection was made.	I really liked how households were selected for cash assistance in this project because it was based on everyone's opinions and the households were surveyed accordingly.
6	I don't know how they determined it, so I have no idea about the process.	Through a lottery in a neighborhood meeting.
7	I do not know how eligibility selection program of providing aid has been set.	The poor were selected in a community gathering.
8	I don't know how eligibility for cash assistance was determined in this program; I've never heard of it.	"It was decided through the meeting with everyone's opinion because I was at the meeting so I know.
9	I am not aware of this process. But I heard that they provided money through mobile.	I think that the rich and the poor in the area, all together chose who is the richest, who is the poorest, and the poorest were given the money.
10	I don't know how the eligibility was determined but many others and I received monetary aid through the process.	She heard that assistance will be provided to 20% of people living in poverty, but no further details have been shared.

Notes: This table displays the open-ended answers provided by ten randomly selected survey responses to the question "How do you think the program determined if someone were eligible to receive cash aid? Why do you think this?"

Table S6: Beneficiary satisfaction and perceptions of fairness

	(1)	(2)	(3)	(4)
	Share satisfied or very satisfied		**Share viewing process as fair**	
CBT (relative to phone-based)	0.265***	0.094*	0.352***	0.119
	(0.027)	(0.052)	(0.038)	(0.095)
CBT * Beneficiary of CBT		0.338***		0.098
		(0.041)		(0.061)
CBT * Beneficiary of Phone		-0.444***		-0.457***
		(0.042)		(0.061)
CBT * CBT meeting participant		0.154***		0.134**
		(0.035)		(0.053)
CBT * Aware of CBT program		0.324***		0.512***
		(0.053)		(0.088)
CBT * Aware of phone program		-0.251***		-0.300***
		(0.039)		(0.061)
N	2,026	2,026	2,026	2,026

Notes: this table presents results from regressions showing correlates of beneficiary satisfaction and perceptions of fairness, for CBT-based vs. phone-based targeting approaches. The main coefficient of interest, **CBT**, indicates whether satisfaction is systematically higher for questions about the CBT relative to the same question about phone-based targeting (estimated using equation (2) (columns (1) and (3)) or equation (3) (columns (2) and (4)). All specifications include household fixed effects to isolate differences in perceptions of the two processes, for a given respondent. All regressions use sample weights to account for sample stratification.

E National Social Assistance Budgets and Scope

Table E1: Budgets and recommended targeting methods for real-world social assistance programs

Country	Year	SA budget (mill. USD)	Households (mill)	Budget per HH (USD)	Best method (BD data)	Best method (TG data)
Panel A: Social assistance in Bangladesh (based on World Bank (2021)						
Typical single program	2019	$30-311	41	$0.73-7.59	Phone-based	Phone-based
Entire SA budget	2019	$1,900	41	$46.34	PMT	Phone-based
Panel B: Social assistance elsewhere (based on World Bank ASPIRE database)						
Guinea-Bissau	2015	$0.10	0.24	$0.43	Phone-based	Phone-based
Sao Tome and Principe	2017	$0.06	0.05	$1.22	Phone-based	Phone-based
Togo	2020	$2.99	2.37	$1.26	Phone-based	Phone-based
Myanmar	2016	$12.64	9.96	$1.27	Phone-based	Phone-based
Papua New Guinea	2015	$2.17	1.31	$1.66	Phone-based	Phone-based
Madagascar	2020	$19.58	6.24	$3.14	Phone-based	Phone-based
Cameroon	2016	$10.14	3.14	$3.23	Phone-based	Phone-based
Somalia	2016	$14.78	2.11	$7.00	Phone-based	Phone-based
Tanzania	2016	$74.66	7.71	$9.68	Phone-based	Phone-based
Lao P.D.R	2021	$16.94	1.70	$9.95	Phone-based	Phone-based
Niger	2017	$46.98	2.87	$16.40	PMT	Phone-based
Zambia	2016	$41.92	2.51	$16.72	PMT	Phone-based
Congo, D.R.	2016	$252.52	13.48	$18.73	PMT	Phone-based
Uganda	2016	$119.74	6.34	$18.90	PMT	Phone-based
Samoa	2016	$0.68	0.03	$22.28	PMT	Phone-based
Rwanda	2020	$70.19	2.93	$23.93	PMT	Phone-based
Burundi	2021	$53.85	2.03	$26.48	PMT	Phone-based
Zimbabwe	2015	$67.87	2.50	$27.20	PMT	Phone-based
Kenya	2017	$287.13	10.33	$27.80	PMT	Phone-based
Ethiopia	2017	$572.40	18.24	$31.37	PMT	Phone-based
Honduras	2018	$74.61	2.34	$31.90	PMT	Phone-based
Sierra Leone	2019	$36.28	1.13	$32.24	PMT	Phone-based
Comoros	2016	$4.05	0.12	$34.59	PMT	Phone-based
Benin	2020	$59.61	1.55	$38.38	PMT	Phone-based
Central African Republic	2015	$34.76	0.88	$39.61	PMT	Phone-based
Mali	2021	$117.79	2.60	$45.29	PMT	Phone-based
Congo, Republic of	2021	$63.75	1.32	$48.47	PMT	Phone-based
Cambodia	2015	$142.59	2.90	$49.13	PMT	Phone-based
Mozambique	2021	$310.43	6.29	$49.38	PMT	Phone-based
Tajikistan	2021	$68.82	1.34	$51.54	PMT	PMT
Pakistan	2021	$1,428.92	27.41	$52.14	PMT	PMT
Guinea	2015	$74.75	1.38	$54.08	PMT	PMT
Uzbekistan	2017	$446.99	7.88	$56.73	PMT	PMT
Indonesia	2016	$3,261.57	54.49	$59.86	PMT	PMT
Angola	2021	$325.88	5.16	$63.13	PMT	PMT

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Table E1 – continued from previous page

Country	Year	SA budget (mill. USD)	Households (mill.)	Budget per HH (USD)	Best method (BD data)	Best method (TG data)
Moldova	2017	$105.66	1.62	$65.11	PMT	PMT
Djibouti	2019	$8.96	0.14	$65.95	PMT	PMT
Tunisia	2019	$201.15	3.04	$66.19	PMT	PMT
Afghanistan	2020	$221.51	3.33	$66.57	PMT	PMT
Burkina Faso	2016	$174.53	2.60	$67.21	PMT	PMT
Bangladesh	2019	$2,704.54	38.26	$70.68	PMT	PMT
Nepal	2021	$590.80	7.06	$83.71	PMT	PMT
Sudan	2016	$607.37	6.64	$91.49	PMT	PMT
Philippines	2016	$1,752.45	18.87	$92.85	PMT	PMT
Vietnam	2016	$2,725.22	25.03	$108.89	PMT	PMT
Kiribati	2016	$2.30	0.02	$113.22	PMT	PMT
Senegal	2015	$138.64	1.12	$123.45	PMT	PMT
Kyrgyz Republic	2018	$213.39	1.56	$136.75	PMT	PMT
India	2016	$32,815.60	228.06	$143.89	PMT	PMT
Thailand	2020	$3,903.57	26.24	$148.76	PMT	PMT
Azerbaijan	2020	$256.16	1.69	$151.46	PMT	PMT
Mauritania	2016	$115.82	0.71	$163.42	PMT	PMT
Ecuador	2015	$1,012.76	5.79	$175.05	PMT	PMT
Bhutan	2021	$26.85	0.15	$178.61	PMT	PMT
Fiji	2016	$31.06	0.17	$180.45	PMT	PMT
Jamaica	2018	$193.49	0.96	$201.41	PMT	PMT
Jordan	2021	$462.96	2.10	$220.22	PMT	PMT
Dominican Republic	2021	$942.43	4.10	$229.61	PMT	PMT
Paraguay	2017	$499.16	2.13	$233.91	PMT	PMT
Armenia	2017	$162.54	0.65	$250.14	PMT	PMT
Guatemala	2020	$419.66	1.67	$250.80	PMT	PMT
Serbia	2020	$634.94	2.47	$257.28	PMT	PMT
Lesotho	2017	$128.45	0.50	$258.14	PMT	PMT
Türkiye	2019	$6,468.55	24.81	$260.75	PMT	PMT
Bolivia	2015	$627.00	2.40	$261.16	PMT	PMT
Mexico	2020	$12,440.23	46.91	$265.20	PMT	PMT
Mongolia	2016	$242.64	0.85	$287.04	PMT	PMT
Malaysia	2016	$1,717.16	5.58	$307.54	PMT	PMT
Ukraine	2021	$10,807.33	34.57	$312.65	PMT	PMT
Belarus	2017	$1,269.63	3.98	$319.09	PMT	PMT
Egypt, Arab Republic of	2020	$8,175.32	23.88	$342.39	PMT	PMT
El Salvador	2019	$365.58	1.04	$350.26	PMT	PMT
North Macedonia	2020	$216.36	0.61	$355.75	PMT	PMT
Colombia	2020	$4,430.48	11.94	$370.99	PMT	PMT
China	2016	$117,949.79	314.61	$374.91	PMT	PMT
Peru	2021	$2,192.43	5.73	$382.47	PMT	PMT
Albania	2020	$283.54	0.73	$386.02	PMT	PMT
Algeria	2021	$3,727.17	9.35	$398.57	PMT	PMT
Iraq	2021	$2,679.22	6.62	$404.77	PMT	PMT

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Table E1 – continued from previous page

Country	Year	SA budget (mill. USD)	Households (mill.)	Budget per HH (USD)	Best method (BD data)	Best method (TG data)
Brazil	2018	$24,536.75	57.45	$427.08	PMT	PMT
Montenegro	2020	$83.47	0.19	$436.22	PMT	PMT
Chile	2018	$10,443.77	23.59	$442.73	PMT	PMT
Timor-Leste	2016	$87.75	0.18	$482.64	PMT	PMT
Kazakhstan	2017	$2,702.25	5.23	$516.43	PMT	PMT
Bosnia and Herzegovina	2017	$509.47	0.90	$565.60	PMT	PMT
Morocco	2021	$2,623.63	4.55	$576.49	PMT	PMT
Panama	2015	$448.96	0.78	$578.21	PMT	PMT
Uruguay	2015	$657.56	1.03	$640.20	PMT	PMT
Georgia	2020	$1,059.89	1.09	$971.09	PMT	PMT
Namibia	2018	$384.46	0.39	$975.46	PMT	PMT
Maldives	2021	$87.22	0.08	$1,031.37	PMT	PMT
South Africa	2020	$15,595.23	11.22	$1,389.44	PMT	PMT
Botswana	2019	$496.76	0.34	$1,445.32	PMT	PMT
Mauritius	2015	$391.44	0.23	$1,671.38	PMT	PMT
Trinidad and Tobago	2018	$911.51	0.46	$1,981.55	PMT	PMT

Notes: In Panel A, data on budgets are taken from World Bank (2021) and data on households is taken from the 2022 population and housing census. In Panel B, be start with data on country social protection budgets as a share of GDP in 2015-2021 from the World Bank's Aspire database (`https://www.worldbank.org/en/data/datatopics/aspire`). We match these with data on yearly GDP and population from the World Bank Open Data (`https://data.worldbank.org/`), as well as survey-based data on average household size from the Global Data Lab (`https://globaldatalab.org/`). The intersection of these three data sources contains information for 95 countries allowing us to calculate an estimate of the social protection budget per household per household screened. The preferred targeting methods are determined by on our calculations of cost-effectiveness incorporating only variable costs for targeting methods, as described in Section 4 and shown in Figure 7. The second-to-rightmost column uses our welfare calculations based on Bangladesh data to identify the best targeting method, while the rightmost column uses our welfare calculations based on Togo data.